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IN THIS ISSUE

INFANT mortality in the United States had declined to the low level of 26 per 1,000 live births in 1956 and was only 23.2 for the white population. A question frequently asked is "Are we approaching an irreducible minimum in mortality in the first year of life?" In the article entitled "The Lower Limit of the Infant Mortality Rate in the United States," C. A. McMahan examines the major causes of death among infants and estimates attainable minimum levels. He concludes that it is not unreasonable to expect rates in the foreseeable future in the range between 7.6 and 12.3 per 1,000 births.

• • •

Surveys of population groups are used extensively to study social and health problems, but frequently interpretation of results is made difficult by the failure to obtain cooperation from or to locate some members of the population. Is the person who refuses to be interviewed different from others and do these "missed" persons cause the results to be not representative of the total group? These questions of bias in survey data and of characteristics of non-cooperators are discussed by Ann Cartwright for a study conducted in a housing development near London in the paper entitled "The Families and Individuals Who Did Not Cooperate on a Sample Survey." In this study persons and families who refused to give more than one interview differed to some extent from those who completed a series of interviews, but information from other sources for those who refused to be interviewed or were not located indicates that they were more similar to the persons who completed the interviews than to those who did not. Although sources of bias and factors affecting cooperation vary greatly for different sur-

veys, this report by Miss Cartwright should be helpful to others in searching for sources of bias.

• • •

In recent years various population surveys have included questions on ideal number of children, number of children desired, and number of children expected. The April, 1955, issue of the *Quarterly* carried an article based on materials of this type collected in the Detroit Area Survey, which is conducted periodically by the University of Michigan. Since the recent economic recession was believed to be particularly severe in Detroit, the authors of the earlier report designed a study to test the impact of the recession on the "expected size of family" reported in 1955. The results are described in the paper by Doctors David Goldberg, Harry Sharp, and Ronald Freedman "The Stability and Reliability of Expected Family Size Data."

• • •

A paper by Dr. Robert Gutman, "Birth and Death Registration in Massachusetts. IV. The System Attains its Basic Goals, 1870–1900" is the concluding one of this series. The author describes the struggle between the statisticians and public health workers on the one hand and the General Court and Secretary of State on the other regarding the responsibility for and function of the registration of vital events in Massachusetts. "The outcome of the campaign was satisfactory to both physicians and the Government, for the reason that it preserved the conception of the birth and death records as important legal documents and at the same time enabled the records to be used to the maximum degree for studying public health." Doubtless there are differences of opinion regarding the present status of the vital statistics system in Massachusetts. Dr. Gutman's series of articles leaves no room to doubt the pioneering rôle of the Bay State in this field in earlier times.

THE LOWER LIMIT OF THE INFANT MORTALITY RATE IN THE UNITED STATES

C. A. McMahan, PH.D.*

IT HAS often been asserted that the infant mortality rate is a sensitive index of social conditions.[1] If this is true, one would certainly infer that conditions improved tremendously in the United States during the period 1915–1956 (Figure 1).[2] The total rate dropped from 99.9 in 1915 to 26.0 in 1956. In 1956 a rate of 23.2 was reported for the white population and one of 42.1 for the nonwhite population. Among the states, Iowa had the lowest rate with 20.6, but the rural areas of the metropolitan counties of South Dakota reported a rate of 14.6. Assuming that these reported rates are essentially accurate, one may begin to speculate upon the limit to which the infant mortality rate might eventually decline.

A general consensus that the infant mortality rate can be reduced further may be inferred from literally hundreds of statements to this effect throughout the literature. Seldom, however, does one chance upon a numerical estimate of the "irreducible minimum." As an example of the type of assertions to be found, one textbook indicates that infant loss still can be reduced considerably but probably not dramatically.[3] A

* Associate Professor, Louisiana State University School of Medicine. Indebtedness is acknowledged to the following persons: To Professor T. Lynn Smith of the University of Florida for suggesting the need for such a paper as this one, to Professor Thomas R. Ford of the University of Kentucky for critically reading the manuscript, to the author's colleagues at the L.S.U. School of Medicine for many stimulating suggestions, and to Carolyn A. Saunders for computational and cartographic assistance. Obviously the author alone is responsible for the estimates.

[1] Some writers have indicated that the mean birth weight of a country's babies was also a useful measure of a nation's health. *See* Salber, Eva J.: The Significance of Birth Weight. *Journal of Tropical Pediatrics*, June, 1955, 1, p. 54.

[2] Linder, Forrest E., and Grove, Robert D.: VITAL STATISTICS RATES IN THE UNITED STATES: 1900–1940. Washington: Government Printing Office, 1947, pp. 572–573; annual volumes, VITAL STATISTICS OF THE UNITED STATES, Part II, 1941 thru 1944. Washington: Government Printing Office, 1943–1946; and "Infant Mortality," *Vital Statistics—Special Reports, National Summaries*, September 29, 1958, 48, No. 12, pp. 339–340, and 353. Throughout this paper, the limitations arising from incompleteness of registration of both births and deaths have been ignored.

[3] Eastman, Nicholson J.: WILLIAMS OBSTETRICS (eleventh edition). New York:

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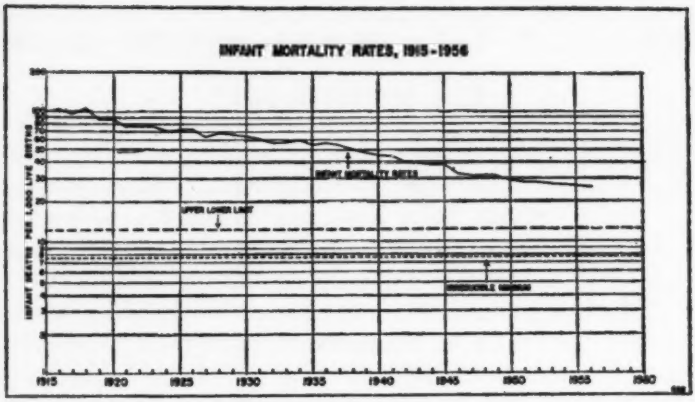

Fig. 1. Infant mortality rates, birth registration states of the United States, 1915–1956 and predicted lower limits.

recent paper states that the present neonatal mortality rate can be reduced considerably by efficient application of present medical knowledge.[4] As a final example of this type of general statement, a 1950 bulletin suggested that ". . . it is not utopian to expect that the infant mortality rate in the United States will be reduced much further before too long."[5]

The hesitancy of writers to attempt to predict numerically the lower limit of infant mortality is understandable.[6] Never-

Appleton-Century-Crofts, Inc., 1956, p. 17. One reason for limitations on infant salvage is the lack of knowledge as to the underlying mechanism which initiates labor prematurely; a second reason is the obscure causes which are responsible for many neonatal deaths; and a third is the lack of medical knowledge concerning the causes of selected complications.

[4] However, to make further progress, according to the authors, basic research must furnish information on prematurity, hyaline membrane disease, intra-uterine infection of the fetus, and fetal erythroblastosis. This same paper reported neonatal mortality rates for 1955 as follows: Norway and Sweden, 13; New Zealand, 14; England and Wales, 17; United States, 19; and Canada, 19. It was stated further that premature infant deaths account for 50 to 75 percent of the neonatal mortality rate. *See* Medovy, Harry and Briggs, J. Nixson: Reduction in Neonatal Mortality—the Present and the Future. In (Alan Ross, editor): Symposium on Recent Clinical Advances, Pediatric Clinics of North America. Philadelphia: W. B. Saunders Co., 1958, pp. 259–277.

[5] Metropolitan Life Insurance Company: How Much Safer Can Maternity and Infancy Be? *Statistical Bulletin*, December, 1950, 31, pp. 8–10.

[6] This paper is in no way a criticism of writers for not furnishing "wild estimates." When the infant mortality rate was "high," the lack of estimates of the

(Continued on page 339)

theless, this paper presents materials which have been formulated in such a manner as to make a quantitative guess at the lower limit of the infant mortality rate, viz., that the infant mortality rate in the United States will not fall below 7.0 in the foreseeable future (and, probably not that low—more likely between 7.6 and 12.3).

Definitions and Notation[7]

Infant Mortality Rate. The infant mortality rate is usually defined as follows:

$$\text{Infant Mortality Rate} = \frac{\text{Number of deaths under 1 year of age reported during a given year}}{\text{Number of live births reported during the same year.}} \times 1{,}000.$$

In order to explain more easily how certain estimates were made, this rate can be stated symbolically as a "specific infant mortality rate":

$$m_i = \frac{d_i^z}{B^z} k,$$

where:

m_i = that part of the infant mortality rate computed from deaths assigned to the ith cause of death,

d_i^z = number of deaths at ages between birth and one year (exclusive of stillbirths) among residents in a community during calendar year z assigned to the ith cause of death,

B^z = total number of live births occurring in the specified community during the same calendar year,

k = a constant usually equal to 1,000 for the total infant mortality rate, but sometimes equal to 100,000 for a specified cause.

lower limit is understandable but it is a little surprising that more estimates have not appeared recently. This is especially true since many demographers seem to feel a compulsion to make rash predictions and of course the estimates suggested in this paper "fit the category."

[7] Notation is similar to that of Linder and Grove, *op. cit.*, pp. 41–47.

Summing over all *i*, the above becomes the infant mortality rate and can be written as follows:

$$m_1 + m_2 + \ldots + m_n = \frac{d_1^* + d_2^* + \ldots + d_n^*}{B^*} k.$$

Since $\sum_{i=1}^{n} m_i = m$ and $\sum_{i=1}^{n} d_i^* = d^*$, then $m = \frac{d^*}{B^*} k$.

Note that the "specific infant mortality rate" m_1 is the number of infant deaths assigned to cause number 1 divided by the number of live births during the calendar year and may be written as follows:

$$m_1 = \frac{d_1^*}{B^*} k.$$

Assumptions

Assumption 1. No marked changes in definitions, statistical classifications, coding, or medical certification. The estimates made in this paper are based upon the state of knowledge and practices during the middle of the Twentieth Century.

Assumption 2. No marked improvement in preventing premature birth. Since this study is limited to live births, it is assumed that there will be no marked improvement in controlling pregnancies which terminate in premature births of 1,000 grams or less.

This is a most questionable assumption and may not be justified by current lack of knowledge. Nevertheless, even if premature births were preventable to a considerable extent, the effect might be to increase the "specific infant mortality rate" subsumed under other assumptions. For example, the number of deaths assigned to congenital malformations might be increased if it is true that many malformed fetuses are delivered prematurely (although there is no certainty that such premature births would result in live births at term).

Assumption 3. Part 1. For births of 1,000 grams or less, m_1 = 4.1. As explained below, it is assumed that at least 4.1 infants

per 1,000 live births will die within the neonatal period.[8] In early 1950 out of 837,786 live births, 3,928 weighed 1,000 grams or less which amounts to about 4.7 per 1,000 live births of all weights.[9] Of these nearly four thousand births, 3,424 or approximately 87 per cent died at an age of less than 28 days. Thus, 4.7 × .87 = 4.1 per 1,000 live births.

Part 2. For births over 1,000 grams, $m_2 = 0.0$ with exceptions as specified under other assumptions. It is assumed that medical care might advance to the place that, if an infant weighs more than 1,000 grams and is not severely malformed, it can and will be kept alive.[10]

Assumption 4. Deaths assigned to congenital malformations, $m_3 = 2.7$. It is assumed that the rate from congenital malformations, as specified, will not decline further. It is further assumed that all infant deaths assigned to the cause known as congenital malformations will occur among infants of less than 60 days of age (suggested for operational use in this paper only).

In 1956 there were 11,451 infant deaths under two months of age assigned to congenital malformations.[11] Since there were 4,218,000 live births, the infant deaths assigned to congenital malformations amount to 2.7 per 1,000 live births.[12]

[8] The neonatal period as used in this paper is the period from birth up to, but not including, the 28th day of life.

The term "pre-viable" has been used to apply to liveborn premature infants with a birth weight of less than 2¾ pounds (about 1,250 grams). Over a six-year period at Simpson Maternity Hospital, 7.9 per 1,000 live births were pre-viable with a death rate of 966 per 1,000. The writer stated that "better figures are attainable, but the mortality of infants weighing less than 2¾ pounds at birth will always be very high." *See* Henderson, J. L.: The Definition of Prematurity. *Journal of Obstetrics and Gynaecology*, February, 1945, 52, No. 1, p. 35.

[9] Shapiro, Sam and Unger, Jeanne: Relation of Weight at Birth to Cause of Death and Age at Death in the Neonatal Period: United States, Early 1950. *Vital Statistics—Special Reports, Selected Studies*, February 23, 1956, 39, No. 6, pp. 249 and 259.

[10] No doubt a rate of m_3 equal to zero will never be attained but the topic of premature infants has been extensively discussed, both philosophically and objectively, in the literature. One example with supporting data is that of Crosse, Mary: Is the Premature Baby Worth Saving? *American Journal of Public Health*, August, 1954, 44, pp. 1010–1014.

[11] "Infant Mortality," *op. cit.*, p. 355.

[12] Natality: United States and Each State, and Alaska, Hawaii, Puerto Rico,

(Continued on page 342)

Limitation. Note that some of the above deaths were from births which weighed 1,000 grams or less and thus might be included under Assumption 3. However, in early 1950 only 30 out of 3,424 deaths whose birth weights were 1,000 grams or less were assigned to congenital malformations.[13] Since this is less than one per cent, the overlap is judged to have relatively little influence.

Assumption 5. Accidents and homicide, $m_4 = .83$ and $m_5 = .03$. It is assumed that the proportion of the total infant mortality rate assigned to accidents and homicide will be no less than it was in 1956. The rates under this assumption have relatively little influence upon the computation of the lower limits but were utilized because the data were easily available.

Part 1. Accidents, $m_4 = .83$. In 1956 there were 3,498 deaths under one year of age assigned to accidents.[14] This is .83 per 1,000 live births.

Part 2. Homicide, $m_5 = .03$. Also in 1956 there were 125 infant deaths assigned to homicide. This is a rate of .03 per 1,000 live births.

Limitation. Possibly some of the deaths considered under Assumption 5 were of infants of 1,000 grams or less at birth. However, since in early 1950 only 29 out of 3,424 deaths of 1,000 grams or less were assigned to "all other causes," including accidents and homicide (that is, the 3,395 other deaths were as-

and the Virgin Islands (U. S.), 1956. *Vital Statistics—Special Reports, National Summaries,* October 15, 1958, 48, No. 14, p. 396.

There is some evidence that this rate is too high. As a matter of interest only, 5,067 births were reported by the L.S.U. Perinatal Committee during a 12-months period of 1957–1958. Nine deaths (which occurred during the period of observation in the hospital) were assigned to congenital malformations yielding a "rate" of 1.8 per 1,000 live births. The records of seven of these deaths were reviewed (limitations recognized) independently by two members of the Pathology Department and the following question answered: "Is there a severe congenital abnormality incompatible with life?" One of the pathologists reported "yes" on five of the seven cases; the other reported "yes" on four of the same group.

On the other hand, it was noted in Australia that in the present state of medical knowledge, no truly effective measures can be used against the class of deaths known as congenital malformations. Thus the 4 deaths per 1,000 in Australia from this cause form a lower limit to the infant death rate. *See* Lancaster, H. O.: Mortality from Congenital Malformations in Australia. *Medical Journal of Australia,* September 8, 1951, 11—38th Year, No. 10, pp. 318–320.

[13] Shapiro and Unger, *op. cit.,* p. 258.

[14] "Infant Mortality," *op. cit.,* p. 356.

signed to the broad classes of "congenital malformations" and "certain diseases of early infancy"), it is clear that there is little duplication.[15]

Estimated "Irreducible Minimum" Level of the Infant Mortality Rate

It should be obvious from the discussion up to this point that this estimate is predicated on the idea that as the infant mortality rate has fallen, the relative importance of specified "causes of death" has increased. Therefore, on the basis of the foregoing assumptions, it is estimated that on the average for each 1,000 live births, at least the following "cause specific infant mortality rates" will prevail:[16]

1. Due to such small size, they lack sufficient strength to survive	$m_1 = 4.1$,
2. Due to size of more than 1,000 grams (with exceptions)	$m_2 = 0.0$,
3. Due to congenital malformations, life beyond one year (i.e., 60 days) is "impossible"	$m_3 = 2.7$,
4. Due to accidents	$m_4 = .83$,
5. Due to homicide	$m_5 = .03$,
Combined rate: $m_1 + m_2 + m_3 + m_4 + m_5 = m$	$m = 7.6$.

A More Realistic Lower Limit

The "irreducible minimum" infant mortality rate estimated above was based primarily on national vital statistics data. Although not ignoring these data, it is of interest to estimate the lower limit by combining them with assumptions derived from physicians' estimates as to the salvage of infants. Some of the assumptions used above will be used again although rates m_3, m_4, and m_5 are not used explicitly.

Assumptions 1, 2, and 3 will again be utilized. Assumption 3 applies to infants of 1,000 grams or less and the rate, $m_1 = 4.1$.

[15] Shapiro and Unger, *op. cit.*, p. 258.

[16] Obviously, m_1 and m_3 are not true "cause-specific rates" but are classes; m_3 really amounts to an assumption of no deaths for all other causes.

It should be recalled that this rate, m_1, assumes that deaths in this weight group will occur during, and only during the neonatal period.

Assumption 6. Fifty-two per cent of neonatal deaths in the 1,000 to 2,500 gram group and 45 per cent in the 2,500 gram group and above are preventable.[17]

Part 1.—Deaths among infants 1,000 to 2,500 grams, $m_6 = 4.2$. In early 1950, out of 837,786 live births, there were 7,282 neonatal deaths in the group 1,001 (sic) to 2,500 grams.[18] If 52 per cent could be prevented, it would be expected that 48 per cent or 3,495 deaths would occur. This is a rate of 4.2 per 1,000 live births.

Part 2.—Deaths among infants of 2,501 grams or more, $m_7 = 4.0$. Among the same births, there were 6,035 neonatal deaths in the group 2,501 grams or greater. Fifty-five per cent of these deaths would be expected to occur under this assumption producing 3,319 deaths and a rate of 4.0.

Assumption 7 (Partially Redundant). No deaths after the neonatal period. It is assumed that there will be no deaths to infants if they live to the age of 28 days or more.

Lower Limit. If it is assumed that no deaths occur beyond the neonatal period for births of 1,000 grams or less, and if this rate $m_1 = 4.1$ is combined with $m_6 = 4.2$ and $m_7 = 4.0$, the lower limit of the infant mortality rate might be expected to be about 12.3.

Discussion

It is possible to compute as many lower limits as there are combinations of assumptions. In any case, the validity of the estimates is restricted by the validity of the assumptions upon which they are based.

[17] Estimates in this general range were reported as good guesses by Schuyler G. Kohl in Perinatal Mortality in New York City. Cambridge, Massachusetts: Harvard University Press for the Commonwealth Fund, 1955, pp. 18–19. Such responsibility factors for infant deaths as the following were used: inadequate prenatal care, errors in medical judgment, errors in medical technique, unqualified medical attendant, family at fault, intercurrent infection, unavoidable disaster, and unsatisfactory pediatric care.

[18] Shapiro and Unger, *op. cit.*, p. 250.

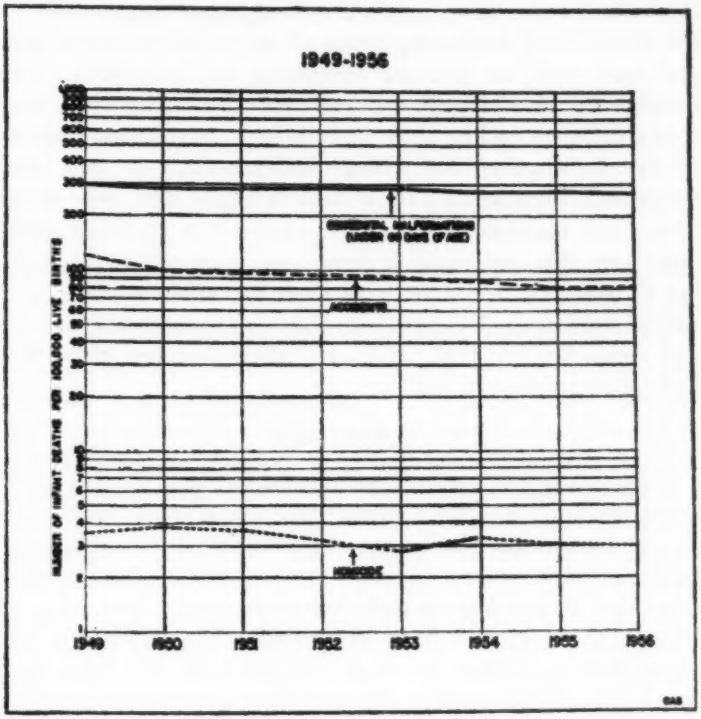

Fig. 2. Number of infant deaths, per 100,000 live births assigned to accidents, congenital malformations (under 60 days of age), and homicide, United States, 1949–1956.

As formulated within this paper Assumptions 3 and 4 are crucial. No doubt some deaths will always occur after the neonatal period. Apparently the death rate assigned to congenital malformations is still declining (see Figure 2) and this is not in agreement with Assumption 4. Because the extent of future decline is unknown, it was decided to use the 1956 rate rather than attempt to extrapolate. Furthermore, the prevalence of congenital malformations may be reduced in the future; for example, a vaccine for German measles might be developed or at least a universal custom of exposing all young females to

the disease before entering child-bearing ages might evolve. On the other hand, increasing exposure to radiation seems likely and may tend to increase congenital malformations. Such possibilities should suffice to remind the reader that there seems to be no end to such speculation. An obvious criticism of the assumption concerning malformations is that some deaths will occur at ages of 60 days or more. The assumptions concerning accidents and homicide are not in complete agreement with the data because these rates seem still to be declining (Figure 2). It might have been better to assume a continued reduction of these latter rates as economic conditions and education improve. However, these last two rates have little effect on the over-all rate.

Conclusion

Several different methods were considered for estimating the lower limit of the infant mortality rate. For the most part, they led to results which were in general agreement. The particular estimates reported in this paper are based upon assumptions that do not appear to be too unreasonable and have the advantage of having been computed from published data. Since these estimates are crude, no correction has been made for deaths of infants born the preceding year or for deaths of infants born during the year of estimate which occur during the succeeding calendar year.

THE FAMILIES AND INDIVIDUALS WHO DID NOT COOPERATE ON A SAMPLE SURVEY

ANN CARTWRIGHT[1]

INTRODUCTION

IT IS sometimes argued that, on surveys concerned with the use of social services, the person who refuses to cooperate is of particular interest, and the enforced exclusion of such people from the inquiry may invalidate the results. One argument advanced in support of this hypothesis is based on the idea of the socially isolated person, who, finding it difficult to make contact with people, does not use the social services and, for the same reason, refuses to answer questions, or sometimes even to come to the door.

In a recent survey we were concerned with the problems of ill-health and the success of the health services in solving these problems, as seen from the viewpoint of the individuals and families concerned (1, 2, 3). It was, therefore, incumbent on us to look at the characteristics of the people who did not cooperate in an attempt to discover whether they were likely to have rather more health problems or different health problems than those who did, and also whether they made more or less use of the health services.

A DESCRIPTION OF THE SURVEY

This survey was part of a larger research program undertaken by the Public Health Department of the London School of Hygiene and Tropical Medicine. The study was carried out in a post-war housing estate just outside London. The population of the estate is about seventeen thousand and, as in many other new housing estates, the population is relatively young, with a high proportion of children and few elderly people (4).

[1] Department of Public Health and Social Medicine, University of Edinburgh. Formerly in the Department of Public Health, London School of Hygiene and Tropical Medicine.

The sample for this study was comprised of the families and individuals living in a randomly selected three-sixteenths of the dwellings on the estate. Our aim was to interview personally all the adults[2] in this sample of dwellings on two occasions at an interval of four weeks. In addition, mothers of school children and children under school age were to be interviewed about their children, on two other occasions, also at an interval of four weeks. This meant that these mothers were interviewed four times in all. In a third of the families they were interviewed about their own health first while in the other two-thirds they were questioned about their children at the first two interviews.

At the first interview we sought information about the subject's health at that point in time. At the second interview we concentrated on events that had occurred between the two interviews: new illnesses, consultations, incapacity, etc. Information concerning the work, education, and family of origin of the adult members of the sample was also obtained at the second interview, but data about the age, sex, and relationships of all the people in the household were obtained at the first contact with the housewife.[3] At the second interview dealing with children, mothers were asked certain questions about their own attitudes towards the estate, and about the friends they had there. Families without children were interviewed on a separate occasion to obtain this information, so that housewives in this group were interviewed on three occasions.

Sources of Information about the Non-Cooperators

It is possible, from the survey itself, to compare information obtained at the first interview about those who cooperated at the second interview and those who did not. In addition, information about the people and families who did not cooperate

[2] Adults were defined as people aged 15 and over who had left school.

[3] In the great majority of families the housewife was the first person contacted and this information was collected straight away to determine how many interviews were needed and for reference in case it was not possible to interview other members of the family.

at all was available from two sources: the London County Council housing records, and the records of the general practitioners on the estate.

The London County Council kept fairly detailed records about the families who moved into dwellings on the estate. This information was kept up-to-date by members of our research team who consulted the local notifications of births and deaths and also recorded any transfer of tenancies. This record provided information about the size and composition of the families and also whether, at the time of their application for a house to the London County Council they put forward any health condition. Houses were allocated on a points system which took account of such things as overcrowding, ill-health, especially tuberculosis, and lack of facilities in the previous accommodation.

There were six doctors practicing on the estate, five in a partnership and the other on his own. About 86 per cent of the people living on the estate were registered with these doctors. All six doctors took part in a study of their work on the estate, using a modified form of the National Health Service record card for each patient. On this they recorded certain details of each consultation for a period of one calendar year.

These two sources of data make it possible to compare non-cooperators with cooperators in respect of age, sex, family composition, health reasons for rehousing, rent record, registration with a general practitioner on the estate, and frequency of consultation with the doctor.

Number of Failures and the Reasons for Not Obtaining Interviews

No interview at all was obtained in 5.5 per cent of our sample of dwellings, all the interviews were completed for 73.2 per cent of the families, and in 21.3 per cent of the families incomplete information was collected, i.e. not all the individuals were interviewed the appropriate number of times. If we consider the individuals living in these dwellings, the corresponding propor-

tions are 14.5 per cent no interview, 4.7 per cent one interview only and 80.8 per cent both interviews completed. This last proportion includes some individuals for whom no information or only incomplete data was obtained about other members of their households or about the family itself. The reasons for failure are shown in Table 1.

The main reason for failure to obtain an interview was a refusal. This does not necessarily mean that an informant told the interviewer specifically that he was unwilling to co-operate, but if, for example, the door was not answered although the interviewer could hear someone inside, or if an appointment

Table 1. Number of failures and the reasons for not obtaining interviews.

	Families		Individuals (Adults and Children)	
	Number	Per Cent	Number	Per Cent
Complete	547	73.2	2,455	80.8
Incomplete	159	21.3	145	4.7
Refusal	122	16.3	104	3.4
Removed	19	2.5	10	0.3
Temporarily Away	8	1.1	7	0.2
In Hospital	—	—	4	0.1
Mother of Child Subject too Ill, in Hospital	—	—	5	0.2
No Contact	2	0.3	3	0.1
Too Ill	—	—	4	0.1
Other Reason	8	1.1	8	0.3
No Interview	41	5.5	440	14.5
Refusal	34	4.6	346	11.4
Removed	—	—	31	1.0[a]
Temporarily Away	—	—	22	0.7
In Hospital	—	—	14	0.5
Mother of Child Subject too Ill, in Hospital	—	—	8	0.3
No Contact	7	0.9	12	0.4
Other Reason	—	—	7	0.2
Total	747	100	3,040	100

[a] In these cases the family had removed after interviews had been completed for either the adults or the children only.

was not kept then this was classified as a refusal. In addition if one member of the family told the interviewer that he did not wish to participate in the inquiry, and the interviewer was unable to persuade him to change his mind, then each member of the family was classified as a failure because of a refusal although the interviewer did not speak to each of them individually. Interviewing individuals in samples of families rather than a sample of individuals such as that obtained from the electoral register is likely in this way, to increase the proportion of refusals.

Reasons for refusal seem to have been very varied. In a number of cases some crisis was involved, the family was about to move, or in two cases someone in the family had recently died. Sometimes interviewers had little or no opportunity to explain the nature and purpose of the inquiry before being told that an individual was not interested. "Why are people always worrying me? I have just about had enough of it what with Jehovah's witnesses and all the rest." More often people said they were busy at the time of the first contact and then failed to keep, often apparently deliberately, a subsequent appointment.

Only half of one per cent of all individuals were not interviewed because they personally were in hospital at the time. The effect of this omission on our estimate of people with in-patient experience during a year is to decrease it from $\frac{223}{2,614} = 8.5$ per cent to $\frac{209}{2,600} = 8.0$ per cent which does not represent a statistically significant difference. Nevertheless this loss may create a definite bias in that we have no further information about the health of these fourteen people, and their use of services.

The thirteen children for whom either no information or incomplete information was obtained because their mother was too ill or in hospital are another potential source of bias. They may be less healthy, have more nervous symptoms and be less

well cared for than children with more robust mothers. However because their mother could not be interviewed it was not possible to get comparable data for these children, as we had found on a pilot inquiry that information obtained from different sources varies very considerably (5). In our survey which was concerned with general levels of health and use of services, the numbers involved here were too small to be of any great significance; but, in an inquiry whose main purpose was for example the identification of children who might be neglected or socially deprived, further investigation would need to be made in these cases.

Failure to obtain interviews because people have moved, is a problem more commonly associated with samples of individuals selected from the electoral register than with samples of families living in particular dwellings. Our failure here arises solely from our desire to interview members of our chosen families over a period of time. It is not a large problem nor one which is likely to have any very definite biasing effect on our inquiry, although it is possible that the people who move from a new estate may be rather different from those who stay.

The most frequent reason for individuals being temporarily away from home was their work. In a very few cases the family was on holiday, but we did not attempt any interviewing in the school holiday period.

Apart then from two small groups of people who were in hospital and children whose mothers were ill, the main source of bias on this inquiry is likely to be due to our failure to obtain information from the families and individuals who refused to cooperate.

The Age, Sex and Marital Status of Individuals for Whom Incomplete Data Were Obtained

The proportion of complete successes was greater for children, (87 per cent) than for adults (77 per cent). There was little or no variation in this latter proportion for males and

	Males		Females		Both Sexes	
	Per Cent	Number[1]	Per Cent	Number	Per Cent	Number
Status Group						
Pre-School Children	85	181	89	164	87	345
School Children	85	428	88	446	87	874
Adults						
Single	68	157	64	183	66	340
Married	80	694	80	700	80	1,394
Widowed/Divorced	*	14	70	70	70	84
All Adults	77	865	76	954	77	1,820
All Status Groups	81	1,474	81	1,564	81	3,040

* Numbers too small to estimate percentage.
[1] The numbers are totals on which the percentages are based. There were two adults for whom the status and one for whom the sex was not known. For one child it was not known whether it was at school or under 5 years old.

Table 2. Proportion of individuals in sex and status groups for whom two interviews were obtained.

females, but for both men and women the proportion of failures was considerably greater among single adults than for people who were married. (Table 2.)

When the ages of the adults are considered, the success rate is highest for the age group 25–44. The difficulty of obtaining interviews from young single adults (82 per cent of the single

Table 3. Success rate for adults in different age groups.

Age Group	All Adults		Married Adults Only	
	Per Cent with Two Interviews	Number of Individuals	Per Cent with Two Interviews	Number of Individuals
Under 25	70	310	94	34
25–34	85	473	88	436
35–44	83	558	84	540
45–54	70	267	71	253
55–64	68	83	71	107
65 and Over	74	96		
Not Known	—	33	—	24
All Ages	77	1,820*	80	1,394

* There were 33 adults for whom the ages were not known. It is possible that these contained a high proportion of old people and that if they were included the success rate for those aged 65 and over would be appreciably lower.

	Complete Success		At Least One Interview		Sample as Drawn	
	No.	Per Cent	No.	Per Cent	No.	Per Cent
Children	1,056	43	1,113	43	1,220	40
Adults	1,399	57	1,487	57	1,820	60
Total Number	2,455		2,600		3,040	
Marital Status of Adults						
Single	224	16	253	17	340	19
Married	1,116	80	1,168	79	1,394	76
Widowed or Divorced	59	4	66	4	84	5
Total Number	1,399		1,487		1,818	
Age of Adults						
Under 25	218	16	238	16	310	17
25–34	402	29	414	28	473	26
35–44	463	33	474	33	558	30
45–54	187	13	214	14	267	15
55–64	56	4	64	4	83	5
65 and Over	71	5	81	5	96	5
Unknown	2	—	2	—	33	2

Table 4. Effect of failures on the sample achieved.

adults were under 25) is well known among interviewers, but among married people the success rate declines with increasing age. (Table 3.)

Failure rates then tend to be higher for adults than for children and single adults are less likely to be interviewed than married people. The effect of these differences on the sample achieved is shown in Table 4.

Normally comparisons can only be made between the sample achieved and population estimates. If we use our sample as drawn to provide estimates of the expected number of single and other adults among those we interviewed at all we find $X_1^2 = 2.77$ $(.05 < p < .10)$. However, we know the actual numbers of single people and others whom we failed to interview at all, and if we calculate our X^2 with these data it is $X_1^2 = 15.19$ $(p < .01)$. This shows how comparisons which are normally made between the sample achieved and population estimates will not necesarily reveal existing biases among people who could not be interviewed.

The Reported Health of Individuals for Whom Only One Interview Was Obtained

It is possible to compare information obtained at the first interview about those who were and those who were not in-

Table 5. Reported health at first interview of women who completed the second interview and those who did not.

	Adult Females	
	One Interview Only	Two Interviews
Average Number of Illnesses Reported at First Interview	3.3	3.3
Average Number of Symptoms	5.3	4.9
Proportion Reporting	per cent	
No Illness	9	9
Backache	27	30
Breathlessness	18	25
Catarrh	18	27
Colds	20	23
Constipation	18	15
Coughing	9	17
Depression	25	23
Dizziness	22	15
Eyestrain	38	22
Headaches	33	39
Indigestion	18	16
Nerves	38	29
Painful Joints	20	18
Palpitations	25	17
Rheumatism	35	28
Sleeplessness	20	16
Stomach Pains	18	10
Swollen Ankles	18	18
Teeth	11	14
Undue Irritability	20	12
Undue Tiredness	16	16
Varicose Veins	18	18
Weak or Painful Feet	20	15
Women's Complaints	27	26
Worried About Their Health	25	23
Number of Individuals = 100 Per Cent	55	728

terviewed a second time. Rather more adult women than men completed only one interview, and since in addition the proportion of proxy interviews was high among men for whom only one interview was obtained,[4] the comparisons for adults were made for women only. (Table 5.)

There was no difference in the average number of illnesses reported by women who gave only one interview and women who were interviewed twice, and in each group similar proportions reported no illness at all. Of the twenty-four symptoms most commonly reported (i.e. by more than 10 per cent of adults) only one, eye strain, differed significantly in the frequency of occurrence in the two groups, and indeed one in twenty-four becomes insignificant. It could in any case be attributed to the slightly different age composition of the two groups.

It might have been expected that the psychosomatic symptoms would be more common in the group who only gave one interview and both "nerves" and "undue irritability" are in fact reported by a higher proportion of individuals in this group. However, the probability of the differences occurring by chance is greater than 1 in 10 in the first case and than 5 in 100 in the second. There was no significant difference either in the proportion who said they were worried about their health.

There is no clear evidence that the health of adult women for whom only one interview was obtained differed very greatly from those who gave two interviews.

For children however we find fewer illnesses reported for those with only one interview, than for those with two interviews.[5] A relatively high proportion in the former group had no reported illnesses. Looking at particular conditions, the difference between the two groups was greatest for catarrh, colds, coughs, and trouble with teeth or gums. For running ears or

[4] Seven out of 31 men with only one interview had proxy interviews compared with an overall rate of 7 per cent proxy interviews. Proxies were accepted only when the alternative was for the person to be omitted from the inquiry.

[5] The proportion of children under 5 years of age in the two groups was 25 per cent and 28 per cent respectively.

earache, possibly one of the more definite conditions, there was no significant difference.

Two possible and rather contradictory explanations of this difference occur to one immediately. One is that mothers with healthy children are less anxious about their children's health and less prepared to give up time to discuss it at a second interview. The second possibility is that the mothers who gave only one interview are less aware of ill-health in their children, or less inclined to regard coughs and colds as illnesses worth reporting at an interview.

We have found no evidence from the adult women themselves to suggest that healthy people are less willing to cooperate at a second interview, but mothers' attitudes towards

Table 6. Reported health at first interview for children for whom a second interview was obtained and those for whom it was not.

	Children	
	One Interview Only	Two Interviews
Average Number of Illnesses Reported at First Interview	0.7	1.3
Proportion Reporting:	per cent	
No Illness	44	33
Catarrh	7	19
Colds	14	21
Coughing	9	14
Eyestrain	5	9
Headaches	7	9
Kidney Trouble or Trouble Passing Water	5	3
Loss of Appetite	5	8
Nerves	12	13
Rashes or Itches	5	6
Running Ears or Earache	9	7
Sore Throat	4	7
Stomach Pains	5	6
Trouble with Teeth or Gums	2	9
Unusual Bleeding	5	4
Number of Individuals = 100 Per Cent	57	1,056

and willingness to discuss their own health may be rather different from their attitude towards and willingness to discuss their children's health. On the other hand the differences between particular conditions shown in Table 6 are not inconsistent with the second theory.

Further information on this point is available from the records of the general practitioners on the estate.

Use of General Practitioners on the Estate

An analysis of the records of the six general practitioners on the estate for the year 1953 showed that 86 per cent of the people living on the estate were registered with these doctors for some part of that year. The proportion of the individuals in our sample who were included in this study of general practitioners records are shown in Table 7.

A relatively high proportion of adults who did not cooperate at all, were not registered with general practitioners on the estate. Unfortunately, we do not know whether these people were registered with other doctors nearby or whether they were still going to their former doctors in London, or whether they had just not been ill since they came to live on the estate and

Table 7. Proportion of individuals living on estate who were registered with doctors on the estate during 1953.

	Adults			Children		
	No Interview (Per Cent)	One Interview Only (Per Cent)	Two Interviews (Per Cent)	No Interview (Per Cent)	One Interview Only (Per Cent)	Two Interviews (Per Cent)
Registered Whole Year	74	84	82	76	76	79
Registered Part Year	3	7	3	2	—	7
Not Registered with G.P.s on Estate During 1953	23	9	15	22	24	14
Sample[1] = 100 Per Cent	314	87	1,298	101	46	992

[1] The sample figures are slightly lower than those in Table 4 as only individuals living on the estate during 1953 have been included.

had not bothered to re-register for this reason. If they were registered and likely to go to doctors not on the estate, it would seem possible that these people were less "estate orientated" and more likely to reject an inquiry based specifically on the estate. If they were not registered, or just had not bothered to change their doctor since they came to the estate, it may be that they were not interested in health or alternatively did not like discussing illness, and for either reason they might refuse to participate in a health survey. Table 8 shows the frequency with which adults and children in our three groups who were registered with these general practitioners throughout the study year, consulted their general practitioner.

For adults, the group that stands out is that with only one interview. These people tended to consult their general practitioners more frequently than adults in either of the two other groups. Although there were more women than men in this group, an analysis by sex showed that for both men and women the average was unexpectedly high in this group. In contrast, those with no interview at all show a very similar distribution to those who completed both interviews.

Table 8. Frequency of consultation with general practitioner of individuals registered with G.P.s on estate throughout 1953.

No. of Consultations	Adults			Children		
	No Interview (Per Cent)	One Interview Only (Per Cent)	Two Interviews (Per Cent)	No Interview (Per Cent)	One Interview Only (Per Cent)	Two Interviews (Per Cent)
None	25	17	24	31	25	19
One or Two	24	15	28	26	55	32
Three–Five	24	32	23	31	17	27
Six–Eleven	18	19	17	5	3	18
Twelve or More	9	17	8	7	—	4
Average Number of Consultations	4.3	5.9	4.0	2.6	1.7	3.5
Sample = 100 Per Cent	233	73	1,062	77	36	790

For children the group with only one interview also showed the greatest variation from the general average, but here the average number of consultations was unexpectedly low. The number on which this average is based is small, but the difference between those with one interview and those with two is statistically significant. In addition the proportion of children with no consultations rose from a fifth of those with two interviews, to a quarter of those with only one interview and to nearly a third of those with no interview at all.

The smaller use of the doctor for children with one interview only is consistent with our earlier finding that mothers reported fewer illnesses for such children, but here again it is only possible to speculate on the possible reason for these differences. Do their mothers take these children to the doctor less frequently because they are, in fact, less ill, or do their mothers have a "higher threshold" of ill-health, being less inclined to regard certain conditions both as worth consulting a general practitioner about, and as worth reporting as illnesses to our interviewers?

In the case of adults however, those interviewed only once consulted their general practitioners more frequently than other adults although they reported similar numbers of illnesses. It would seem that these adults may be using their general practitioner in a rather different way. This possibility is examined in Table 9, which shows the average number of illnesses the general practitioners recorded for adults in the three groups

Table 9. Number of illnesses recorded by general practitioners for adults registered with G.P.s on estate throughout 1953.

	No Interview	One Interview Only	Two Interviews
Average Number of Illnesses Recorded by G.P.	2.4	2.7	2.5
Average Number of Consultations per Illness	1.8	2.2	1.6
Sample	233	73	1,062

and gives an estimate of the average number of consultations per illness.

Although there is some indication that the average number of illnesses is slightly higher for the individuals with only one interview, the differences in these figures are small. Somewhat larger differences are apparent when we consider the average number of consultations per illness. This would appear to indicate that the adult "partial cooperators" resembled the adults in the other two groups in their number of illnesses but consulted their general practitioners rather more frequently.

Health Reasons for Moving to Housing Estate

Some further information about the health of individuals in our three groups was obtained from the London County Council housing records. These indicated the families who had put forward some health reason to support their application for rehousing on the estate, together with the individuals who were involved.

Again the individuals with only one interview stand out, and once again this group of adults would appear to be less healthy than the other groups while the children in this group are apparently more healthy (Table 10).

Unfortunately, we cannot regard this information as objective evidence that the children with one interview were in fact more healthy since it only indicates that their parents did not

Table 10. Health reasons put forward for move.

	Adults			Children		
	No Interview (Per Cent)	One Interview Only (Per Cent)	Two Interviews (Per Cent)	No Interview (Per Cent)	One Interview Only (Per Cent)	Two Interviews (Per Cent)
Yes	20	33	22	20	9	16
No	80	67	78	80	91	84
Sample = 100 Per Cent	333	88	1,399	107	57	1,056

put forward their ill-health as a reason for being rehoused. There is no particular reason however for thinking that this group of parents would be less likely to use such information in their application for rehousing, and certainly there is no reason here for rejecting the hypothesis that children in this group are relatively healthy, although the alternative hypothesis, that their mothers are less aware of their ill-health cannot be completely dismissed.

Again we can only speculate on the reasons for the differences among adults. Those who gave only one interview, if they had had an illness in the past serious enough to be given medical priority for rehousing but from which they had now recovered, might, quite reasonably, consult their general practitioner more frequently when they were ill. They might also be reluctant to be interviewed again if they felt they were living on the estate "on false pretences" as they were no longer ill.

Families for Whom Incomplete Data Were Obtained

So far we have considered the effect of failure to obtain inter-

Table 11. Amount of data obtained for families.

Adult Interviews	Child or Family[1] Interviews	Families with Children Under 15 (Per Cent)	Families with No Children Under 15 (Per Cent)	All Families (Per Cent)
Complete	Complete	78	61	73
Incomplete	Complete	3	13	6
None	Complete	7	7	7
Complete	Incomplete	1	—	1
Complete	None	3	4	3
Incomplete	None	4	4	4
None	Incomplete		—	
None	None	4	11	6
Adult Interviews Complete		82	65	77
Child Interviews Complete		88	—	—
Sample = 100 Per Cent		563	184	747

[1] In families with no children under 15, the housewife was interviewed three times, twice about her own health and once about certain family circumstances. This latter interview is described as the family interview.

views on our sample of individuals. But we were also concerned to study the families in our area, and this section shows some of the effects of failure on the sample of families.

It has already been shown that very few families, 5.5 per cent, gave no interview at all. For 21.3 per cent some but not all the interviews were obtained, and for 73.2 per cent all interviews were completed. An analysis of the type of failure involved is given in Table 11.

In 82 per cent of the families with children under 15, all the adults cooperated fully, but this proportion was only 65 per cent in families without any children. The overall response rate was considerably higher in the families with children. This means that there is a considerable difference between the three groups of families, those who cooperated fully, partially, or not at all, in the proportion with children under 15.

	Per Cent With Children Under 15	*Number of Families*
Families Giving No Interview	51	41
Families Giving Incomplete Data	67	159
Families Giving Complete Data	80	547
ALL FAMILIES	75	747

Since the presence or absence of children is likely to affect other characteristics such as the size and composition of a family, Table 12 shows these factors separately for families with and those without children, in the three groups.

There is some indication here that the chance of failure increases with size of family in households with children. However, the composition of the family rather than the numbers in it appears to be a rather more important factor in determining chance of failure. Relatively few families consisting only of a basic family unit, a married couple and their children, were not interviewed at all. Families, on the other hand, which contained other adults living with a married couple either with or without children had a high failure rate. Families with incomplete in-

terviews again fell between the other two groups. This high failure rate among the structurally complicated families may be important in estimating the frequency of occurrence of certain types of problems which are more likely to occur in family units containing, for example, two mothers. One is tempted to surmise that it is the "normal" or more typical group who is more willing to be interviewed.

It might be thought that some refusals to cooperate may have been prompted by a desire to conceal certain facts, such as, for example, a failure to pay the rent regularly. This was not a serious problem for the London County Council on this estate; only 9 per cent of families were in arrears for 10 weeks or more over a period of two years, and in fact this proportion did not vary significantly with cooperation on this inquiry.

Table 12. Variations in the proportion of successes in completing interviews with family size and composition.

Size and Composition of Family	Children Under 15 and Still at School in the Family				Families with No Children Under 15 and Still at School			
	Number of Families (=100 Per Cent)	Per Cent of Families			Number of Families (=100 Per Cent)	Per Cent of Families		
		No Interview	Incomplete Interview	Complete Interview		No Interview	Incomplete Interview	Complete Interview
Size of Family								
One	—				40	13	30	57
Two	2				76	12	28	60
Three	95	2	20	78	46	9	30	61
Four	202	3	18	79	–			
Five	147	3	14	83	–22	9	36	55
Six	72	6	19	75	–			
Seven+	45	9	33	58	–			
Average Size		5.2	4.8	4.6		2.2	2.4	2.3
Family Composition								
Married Couple Only					71	11	28	61
Married Couple and Offspring Only	511	3	18	79	51	6	35	59
No Married Couple	15	(20)	(13)	(67)	48	13	25	62
Married Couple, Other Adults, with(out) Offspring	38	11	22	67	14	(21)	(36)	(43)

Note: Percents in parentheses are based on less than 20 families.

(Table 13.) Not surprisingly families with children were rather more likely to have several weeks of arrears than families without children.

It is possible to compare some of the families who only partly cooperated in the inquiry with those who cooperated fully for certain characteristics ascertained during the interview. Information obtained from the housewife about the family as a whole is given in Table 14 which shows that a relatively high proportion of families who do not cooperate completely regretted that they had come to live on the estate. A possible explanation for this may that some of these families moved away from the estate and failed to complete the interviews for that reason. However, the proportion is still high in families who did not cooperate for other reasons (mainly a refusal) being 10 per cent for families with children and 17 per cent for families without. It seems likely that dissatisfied people are less willing to answer questions than others.

Fewer housewives in the group of families with incomplete data said they had made friends since coming to live on the estate, but this difference is not statistically significant and

Table 13. Rents records of families with complete and incomplete data.

Number of Weeks Rent in Arrears (In Two Years)	Children in the Family			No Children in the Family		
	No Interview (Per Cent)	Incomplete Interviews (Per Cent)	Complete Interviews (Per Cent)	No Interview (Per Cent)	Incomplete Interviews (Per Cent)	Complete Interviews (Per Cent)
None	53	51	48	(77)	64	60
1–9	38	35	43	(23)	32	35
10 or More	9	14	9	—	4	5
Average Number	2.5	3.8	3.0	0.5	1.7	1.6
Number of Families = 100 Per Cent	21	87	398	13	47	96

Note: Figures in parentheses are based on less than 20 families.

	Children in the Family		No Children in the Family	
	Incomplete Interview (Per Cent)	Complete Interview (Per Cent)	Incomplete Interview (Per Cent)	Complete Interview (Per Cent)
On the Whole Would You Say You Are Glad or Sorry You Came to Live Here?				
Glad	73	78	71	71
Sorry	11	6	13	6
Qualified Answer	14	15	13	20
Don't Know	2	1	3	3
Have You Made Any Friends Since You Came to Live Here?				
Yes	61	70	47	53
No	39	30	53	47
Number of Families = 100 Per Cent	62	438	38	109

Table 14. Attitude towards the estate in families who cooperated fully and those who only partially cooperated, based on families for whom information available.

might have occurred by chance in a sample of this size.

One characteristic for which families with incomplete data conformed more closely to the general pattern of the estate than the families with complete interviews was religion. (Table 15). Housewives in the latter group more frequently reported membership of a church other than the Church of England, and more often said they went to church regularly.

Rather surprisingly more housewives in families with complete interviews went out to work than housewives in the other group, but this may be simply because the latter group were older.

It is possible that a number of these differences between the incomplete and complete interviews with families with children derive from this difference in ages of the housewives. Because the housewife in the incomplete group is older she is less

	Incomplete Interviews (Per Cent)	Complete Interviews (Per Cent)
Religion		
Church of England	85	69
Roman Catholic	9	15
Others	6	16
Church Going		
Regular	3	10
Not Regular	94	85
Qualified or Not Answered	3	5
Working		
Full-Time	11	14
Part-Time or at Home	9	25
Not At All	80	61
Number of Families = 100 Per Cent	35	438
	(Per Cent)	(Per Cent)
Age		
Under 30	15	18
30–39	39	52
40–49	40	27
50 or Over	8	3
Number of Families = 100 Per Cent	104	438

Table 15. Certain characteristics of the housewives in families with children who cooperated fully and those who only partially cooperated for families for whom information available.

likely to make friends and not so appreciative of the estate as a place in which to live (6). The implication for our inquiry is that we have to a small extent underestimated the dissatisfactions with the estate. If similar proportions of all families with incomplete data and families who did not cooperate at all were sorry they had come to live on the estate, this proportion would rise from just under 7 per cent to just over 7 per cent for families with children, and from 8 per cent to 9 per cent of families without children.

Summary

Certain information which was available for the entire sample is analyzed for families and individuals who cooperated

wholly, partially or not at all in an interview survey. In addition those who cooperated fully are compared with those who cooperated only partly, in respect of certain data obtained in the course of interviews.

The only bias which existed when the sample achieved was compared with the sample as drawn was in the ratio of adults to children. The proportion of success was greater for children than for adults.

There were, however, several differences between the three groups of full, partial, and non-cooperators. For a number of characteristics it was the partial cooperators who differed from both the full and non-cooperators while the two latter groups were similar. Some possible explanations of the differences are discussed.

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THE STABILITY AND RELIABILITY OF EXPECTED FAMILY SIZE DATA[1]

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AS THE proportion of planned families increases, preferences in family size become an important component of fertility behavior. Accordingly, there is an increasing emphasis in fertility research on family size desires, ideals, and expectations.[2] Recent studies have made use of family size preferences in two ways: (1) as a means of extending cohort fertility to make birth projections,[3] and (2) as a substitute or approximation of completed family size in studies of differential fertility.[4] In the latter case, the use of expected number of children frees the researcher from some of the problems of *ex post facto* design.[5]

The two major uses of expected family size data require two types of evaluations. For purposes of projection, women's estimates of their future fertility behavior can be evaluated on an aggregate basis. How large is the net error in statements concerning expected number of children? As a substitute for completed family size in studies of the social and psychological

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[2] For example, *see* Muhsam, H. V. and Kiser, C. V.: The Number of Children Desired at the Time of Marriage. The Milbank Memorial Fund *Quarterly*, July, 1956, XXXIV, No. 3, pp. 287–312; Freedman, R.; Goldberg, D.; and Sharp, H.: "Ideals" About Family Size in The Detroit Metropolitan Area. The Milbank Memorial Fund *Quarterly*, April, 1955, XXXIII, pp. 187–197.

[3] Freedman, R.; Whelpton, P. K.; and Campbell, A.: FAMILY PLANNING, STERILITY AND POPULATION GROWTH. New York: McGraw Hill, 1959.

[4] Mishler, E. and Westoff, C.: A Proposal for Research on Social Psychological Factors Affecting Fertility: Concepts and Hypotheses. *In* CURRENT RESEARCH IN HUMAN FERTILITY. New York: Milbank Memorial Fund, 1955, pp. 121–150.

[5] By using expected number of children as the dependent variable we can obtain measures of the independent variables prior to the completion of families. However, a study of families which have not completed their fertility presents the problem of evaluating the final effect of independent upon dependent variable.

factors affecting fertility, we are usually concerned with the difference between expectations and behavior among the individual couples. However, even in studies of differential fertility, an argument can be made for considering net fertility differences between groups. Surely, no one can argue that in a process as complex as family building over a period of 15 or more years, expectations and performance for individuals will be identical. For almost any group there is probably a limited range within which voluntary variation is likely to take place so that numerous upward changes may be balanced by downward changes within such a range. These shifts are the consequence of a multitude of influences on the individual, even if there are no fundamental economic and social changes in the society.

Some research dealing with the accuracy of fertility preferences as an indicator of fertility performance has already been undertaken. Westoff, Mishler, and Kelly reported on the difference between statements of desired family size at time of engagement and completed fertility some twenty years later for a panel of 145 fecund, predominantly college-educated, Protestant couples.[6] Desired number of children was an extremely accurate predictor of actual number of children at the aggregate level. Actual and desired fertility differed by less than 5 per cent. However, the authors note that the similarity between desires and behavior for the total group resulted from a series of cancelling errors. When individual couples were considered, the correlation between desires and actual performance was a rather low .26.

The present report represents another attempt at assessing the probable accuracy or stability of statements about expected number of children. It differs from the previous work in a number of ways: the samples are not as homogeneous as the one used previously; the couples are married and for the most part have already had the experience of having some children; and

[6] Westoff, C.; Mishler, E.; and Kelly, E.: Preferences in Size of Family and Eventual Fertility Twenty Years After. *American Journal of Sociology*, 1957, LXII, pp. 491–497.

it deals with statements of expected family size rather than desired family size.[7]

The Data

There are three sources of data used in this report. Surveys conducted in Detroit in 1955 and 1958 will hereafter be referred to as the 1955 or the 1958 study. The third source was a telephone re-interview of the respondents first contacted in the 1955 study. All of the data were collected through the facilities of The University of Michigan's Detroit Area Study and the Program for Research in Population and Human Ecology. Both the 1955 and 1958 studies used probability samples of the tracted area of greater Detroit.[8]

Our major emphasis in this paper is on family size expectations rather than ideals or desires. The difference between the reality orientation of questions on expected number of children, and the element of fantasy in questions about ideals or desires seems to have been clearly recognized by most of our respondents. About one-half of the women in the 1955 and 1958 studies had expectations which differed from their preferences. Even among fecund couples the correlation between desires and expectations was no more than .43. Moreover, it has been observed in a number of studies in the United States and Western Europe, that average ideals or desires are generally higher than either expectations or behavior.

Indirect Assessment of Expectation Accuracy

Perhaps the first question to be raised about the probable accuracy of data on expected number of children should be one

[7] The question used to obtain expected family size was: How many children do you expect to have altogether? Questions on desires usually request the respondent to specify the number of children *wanted* under various sets of conditions.

[8] The sample universe for these studies includes about 87 per cent of the Standard Metropolitan Area population of Detroit. The samples are described in: Takeshita, J.: Selection of A Sample of Dwelling Units for the Detroit Area Study: 1954–55. 1955 (mimeographed); Clausen, A. and Halsted, D.: The Sample Design for the 1958–59 Detroit Area Study. 1959 (mimeographed). The samples were designed and drawn with the assistance of the staff of the Survey Research Center's Sampling Section.

concerning consistency with past trends in fertility. Obviously the potential source of error in statements about expected number of children is located in the number of *additional* births expected by the interviewed women. To what extent, then, are replies to questions about expected additional births within the range of fertility performance recorded among previous cohorts? Freedman, Whelpton, and Campbell were confronted with this question in preparing their cohort projections for the United States. They found that with the exception of the oldest age group (35–39) in their study, the women's estimates of future fertility looked reasonable enough so that they could be used to extend the cohort tables.[9] The data on additional expected births collected in the Detroit sample are very similar to the data from the Growth of American Family Study. An historical comparison of the consistency of expectations with past fertility, however, only enables us to say that the expectations look "reasonable." To assess their accuracy requires additional types of data.

The presence or absence of uniformities in the gross timing patterns of births among older women, and the expected timing patterns for younger women, should present further evidence on the probable accuracy of expectations. Although there have been large differences in completed family size among the major socioeconomic and religious groups in the past, their timing patterns relative to duration of marriage are strikingly similar, at least in the Detroit area. For married women 40 and over, the number of births in the first five years of marriage, taken as a proportion of total births, varies between 45 and 48 per cent among the several groups considered in Table 1, column 1. The same consistency is found with respect to the proportion of total births that took place in the first ten years of marriage, varying between 72 and 77 per cent.

If the estimates of future births among the younger women are realistic, we should expect to find similar uniformities. Since completed family size is unknown, the actual number of

[9] Freedman, R.; Whelpton, P. K.; and Campbell, A.: *op. cit.* Chapter 10.

Selected Characteristics	Births by Marriage Duration as Per Cent of Actual Births (Older Women) and as Per Cent of Expected Births (Younger Women)						
	Women 40 and Older: Births by Given Marriage Duration as Per Cent of Actual Births			Women Under 40: Births by Given Marriage Duration as Per Cent of Expected Births			
	5 Years (Per Cent)	10 Years (Per Cent)	N	5 Years (Per Cent)	N	10 Years (Per Cent)	N
Husband's Occupation							
Blue Collar	46	74	175	44	222	73	126
White Collar	47	75	92	42	126	76	76
Husband's Income							
Under $5,000	45	72	123	44	158	73	84
$5,000 and Over	48	77	144	42	183	76	112
Wife's Education							
Less Than 12 Years	47	74	175	43	157	73	102
12 Years and Over	46	75	92	43	191	76	100
Wife's Religion							
Catholic	47	75	84	41	131	74	69
Protestant	46	74	140	43	195	73	120
Total	46	74	267	43	348	74	202

Table 1. Births in the first five and the first ten years of marriage taken as a percentage of total actual births to women forty and over, and of total births expected by women under forty, by selected characteristics: 1955 study.

births in the first five or ten years of marriage is taken as a proportion of the number of births the women say they will eventually have. The anticipated timing of births among the younger women is almost identical to the actual timing pattern of the women who have already passed through the childbearing period. Once again, we find only slight variations in timing from one group to another. Unpublished data from the Growth of American Families Study show virtually the same results.

Indirect tests such as these give us some confidence about the accuracy of expectation data for younger women. The ulti-

mate test will be in the fertility behavior of these women over the next ten or fifteen years. Since the Detroit study was done in early 1955, we have only a limited amount of information on the subsequent performance of the younger cohorts.

The Re-Interview Study

In 1955 interviews were obtained from 427 married women under 40 years old in the Detroit area. At that time no plan was made for a re-interview study concerning the fertility performance of these women. However, the drastic change in the economic picture in Detroit, beginning in the fall of 1957, provided a natural setting for a test of the stability of expected family size data under a set of changing conditions. It was decided to evaluate expectation data in two ways: first, to locate the original respondents from the 1955 study and reinterview them; and second, to include a series of questions on future fertility performance in the 1958 Detroit Area Study.

The re-interview was carried out by making use of city directories and cross-indexed telephone directories.[10] Funds were not available to cover the large costs of returning to the dwelling units of the original respondents so we tried to gather our information through the telephone interviews and by mail questionnaires.[11] The followup took place exactly three years after the original interview.

Of the 427 potential respondents, 238 women (56 per cent of the original sample) provided us with information on their fertility behavior during the period 1955–1958. The reinterview rate for various sociodemographic groupings is seen in Table 2. Clearly, our re-interview population differs from the original sample. The survivors are heavily loaded in the higher status groups. This situation was partially a function of our inability to trace the non-white members of the 1955 sample. We located

[10] We are indebted to Mr. J. W. Larsen of the Michigan Bell Telephone Company for his assistance in locating the respondents.

[11] Most of the interviews were conducted over the phone. Respondents who could not be located by telephone were sent a mail questionnaire. Less than ten per cent of the questionnaires were returned.

more than six out of ten whites but less than a third of the non-whites. Data not shown here indicate that the same kind of economic selectivity was operative within the white population. Higher status whites were easier to locate than lower status whites.

Table 2. Selected characteristics of the respondents in the 1955 study and the 1955 characteristics of the 1958 reinterview respondents.

Selected Characteristics in 1955	1955 Study (N)	Re-interview Study (N)	Re-interview Rate (Per Cent)	Selected Characteristics in 1955	1955 Study (N)	Re-interview Study (N)	Re-interview Rate (Per Cent)
Live Births				*Husband's Income*			
0	57	24	42	Under $3,000	57	16	28
1	98	51	52	$3,000-$4,999	155	77	50
2	136	84	62	$5,000-$6,999	124	91	73
3	72	49	68	$7,000 and Over	81	53	65
4–8	64	30	47				
				Husband's Occupation			
Additional Births Expected				Semi and Unskilled	173	71	41
				Skilled	97	66	68
0	225	126	56	Clerical and Sales	59	38	64
1	105	65	62	Proprietor, Mgr., Off.	98	63	64
2	68	34	50				
3–5	28	12	43	*Wife's Education*			
				Grade School	62	22	35
Total Expected Births				High School 1–3 Years	124	57	46
0	29	17	59	High School 4 Years	197	130	66
1	43	21	49	College	44	29	66
2	115	60	52				
3	97	61	63	*Wife's Religion*			
4	89	54	61	Catholic	164	109	66
5–8	54	25	46	Protestant	233	112	48
				Other	30	17	57
Fecundity Status							
Probably Fecund	323	177	55	*Wife's Farm Experience*			
Reduced Fecundity	104	61	59	Some	115	45	39
				None	311	193	62
Age in Years							
20–24	83	30	36	*Race*			
25–29	95	57	60	White	350	214	61
30–34	125	74	59	Non-White	77	24	31
35–39	124	77	62				
Duration of Marriage							
0–4 Years	69	32	46				
5–9 Years	159	90	57				
10–14 Years	119	65	55				
15 and Over	80	51	64				
Total	427	238	56	Total	427	238	56

Although the re-interview population is not representative with respect to the social and economic characteristics, it does much better on the demographic side. The 1955 sample had an average of 2.09 live births, and expected to have an additional .80 births. The 1958 re-interview sample had given birth to 2.10 children by 1955 and expected an additional .74 births at that time. Given the deficiencies of the sample, it nevertheless represents one of the few available opportunities for examining the accuracy of the women's predictions.

Actually, there is no reason to believe that women who say they will have X children will have exactly X children. Even without changes in social and economic conditions, there will be many small compensating shifts in expectations and performance under the impact of the large number of forces affecting the individual family growth histories. Of immediate consequence, unanticipated fecundity problems and accidental births will modify the situation.

During the period considered (1955–1958), the economic circumstances had changed considerably in Detroit. In early 1955 only 4 per cent of the adults in the labor force were unemployed. Income rose throughout 1955, 1956, and the first three quarters of 1957. Then in the fall, the automobile and related industry layoffs began. When the sample was re-interviewed, about 11 per cent of the adult labor force was unemployed and another large fraction was experiencing partial unemployment. This situation continued throughout 1958.

At an aggregate level it appears that the depressed conditions in Detroit will have little effect on the total number of children the re-interviewed sample will eventually bear, although it may alter some of the timing patterns. In 1955 the 237 women expected to have an additional 176 children. By 1958 they had experienced 110 pregnancies, 84 live births, and expected to have 94 more births—totalling 178 births, actual and anticipated. Thus there was a net change of only two births in the total number of children expected by the sample. The net error, assuming the anticipated births will eventually take

place, is less than one child per 100 women. This error remains extremely small, amounting to no more than three births per 100 women, when the difference between the 1955 and 1958 expectations is standardized on the characteristics of the original sample (assuming a 100 per cent re-interview rate and that the persons who were re-interviewed from the various sociodemographic categories were similar to the persons who were not re-interviewed in the respective sociodemographic categories).

A net error of virtually zero results from a series of upward and downward revisions in expected number of children by the individual woman. After three years, 30 per cent of the women have changed their total expectations and 8 per cent of the re-interviewed women now have expectations which differ from their 1955 expectations by more than one child. Approximately equal numbers of women revised their estimates upward and downward.

Some of the changes in expected family size result from genuine shifts in the true value which are beyond the control of the respondent (fecundity impairments) rather than from unreliability in the data. It seems to us that a gross change of 30 per cent in a variable as complex as expected family size is remarkably small considering the fact that the interviews were spaced three years apart. Gross errors of this magnitude have occurred in data like education, rent, and income when the re-interview followed some weeks later.[12] Panel studies of consumer expectations, which are probably more comparable to the type of data used in this report, indicate that accurate predictions of aggregate purchases can be made from survey data, but they also show that about half of the persons who expect to purchase major goods such as new cars, furniture, or appliances in the following one year period, fail to make such purchases.[13]

[12] Eckler, A. R.: Extent and Character of Error in the 1950 Census. *The American Statistician*, 1953, VII, pp. 15–21.

[13] *See* Lininger, C.; Mueller, E.; Wyss, H.: Some Uses of Panel Studies in

(Continued on page 378)

Additional Children Expected in 1955	Average Number of Live Births	Average Number of Pregnancies
0	.17	.22
1	.46	.55
2 and Over	.72	1.00

Table 3. Additional births expected in 1955 by average number of live births and pregnancies during the period 1955–1958: Re-interview Study.

In some respects, making use of anticipated births in 1958 to evaluate the accuracy of anticipated births in 1955 is unfair. Perhaps the criteria should be based on fertility behavior between the time of original interview and re-interview. Most of the couples seem to be acting in accordance with their original predictions. Women expecting no additional births have had an average of .17 births while those expecting two or more children have given birth to an average of .72 children over the three-year period (Table 3). In terms of error cases, 17 per cent of the women who predicted no future births have become pregnant and 42 per cent of those predicting some future births have not been pregnant, yielding a total of 29 per cent who may be classified as error cases, for the time being.

An analysis of the 1955 characteristics of the re-interviewed women reveals some conditions which seem to affect the accuracy of the original fertility predictions (Table 4). In the Westoff, Mishler, and Kelly article[14] it was suggested that prediction accuracy was probably a function of experience in the domestic role. This seems to be the case in our data. The percentage of women whose expectations were identical in 1955 and 1958 is directly related to age and duration of marriage. Less than half of the women aged 20–24 or married for less than five years had the same total expectations in 1955 and 1958. Combinations of the "experience" variables yield highly con-

Forecasting the Automobile Market. Paper presented at the 1957 meetings of the American Statistical Association; Lansing, J., and Withey, S.: Consumer Anticipations: Their Use in Forecasting Consumer Behavior. *In* Short Term Economic Forecasting. A Report of the National Bureau of Economic Research. Princeton: Princeton University Press, 1955.

[14] *Op. cit.*

sistent results. For example, 53 per cent of the women married less than ten years, under 30 years old and with one child or less changed their expected family size, whereas 29 per cent of the women married less than ten years, but who are more than 30

Table 4. Changes in expected number of children between 1955 and 1958[1] by selected characteristics as of 1955: Re-interview Study.

Selected Characteristics in 1955	Same in 1955 and 1958 (Per Cent)	More in 1958 (Per Cent)	Fewer in 1958 (Per Cent)	Selected Characteristics in 1955	Same in 1955 and 1958 (Per Cent)	More in 1958 (Per Cent)	Fewer in 1958 (Per Cent)
Live Births				*Husband's Income*			
0	67	12	21	Under $3,000	56	19	25
1	62	22	16	$3,000–$4,999	67	18	15
2	71	18	11	$5,000–$6,999	74	14	12
3	74	10	16	$7,000 and Over	70	17	13
Additional Births Expected				*Husband's Occupation*			
0	83	17	—	Semi and Unskilled	66	17	17
1	62	20	18	Skilled	76	15	9
2	47	15	38	Clerical and Sales	76	16	8
3–5	33	—	67	Proprietors, Mgrs., Off.	64	17	19
Total Expected Births				*Wife's Education*			
0	82	18	—	Grade School	73	23	4
1	85	10	5	High School 1–3 Years	62	25	13
2	73	25	2	High School 4 Years	73	12	15
3	72	15	13	College	65	14	21
4	53	17	30				
5–8	67	4	29	*Wife's Religion*			
				Catholic	70	17	13
Fecundity Status				Protestant	69	16	15
Probably Fecund	68	15	17	Other	70	18	12
Reduced Fecundity	75	20	5				
				Wife's Farm Experience			
Age in Years				Some	67	9	24
20–24	40	40	20	None	70	18	12
25–29	61	25	14				
30–34	70	14	16	*Race*			
35–39	87	4	9	White	70	16	14
				Non-White	67	20	13
Duration of Marriage							
0–4 Years	47	28	25				
5–9 Years	62	25	13				
10–14 Years	73	13	14				
15 and Over	92	—	8				
Total	70	16	14	Total	70	16	14

[1]Same number of cases as given in Table 2.

years old and have two or more children, changed their expectations. In contrast to the demographic variables, the social and economic characteristics seem to have relatively little effect on prediction accuracy.

The women re-interviewed in Detroit were able to give us some indication of the perceived conditions precipitating the changes in their original fertility predictions. When the sum of the births between 1955 and 1958 plus the expected additional children in 1958 did not correspond with the expected additional children in 1955, respondents were told:

> When we last interviewed you, you said that you expected (X) more children. Now it appears that you may have ($X \pm 1$ or 2) more. Can you tell me why you have changed your mind?

Among the respondents who had revised their estimates upward, the primary reasons given were accidental conception and attempts to balance the sex ratio among the children. Other reasons ranged from medical treatment making additional births possible, to having twins, to remarks such as, "I just like to have little babies around." The reasons given for having fewer children were either fecundity problems or economic strain. Seven of the nine women who revised downward by two or more children mentioned some kind of physical difficulty.

Other than fecundity impairments, economic strain was the only reason given by our respondents for downward revisions in the fertility estimates. Yet not a single respondent mentioned economic improvement as a condition which influenced her upward revision. In effect, our respondents told us that the relationship was entirely one-sided. This seemed highly unlikely in view of the strong correlation known to exist between fluctuations in the birth rate and economic conditions. With data on the characteristics of the couple in 1955 and with information on changes in these characteristics since 1955, economic strain and improvement could be measured.

In Table 5, status improvement is defined as a change in the husband's job involving a shift upward in a four unit occupa-

Change in Status	More Expected in 1958 (Per Cent)	Same in 1955 and 1958 (Per Cent)	Less Expected in 1958 (Per Cent)	Total (Per Cent)	Net Change per Person 1958 Less 1955	Number of Cases
Status Improvement	26	59	15	100	+.12	61
Status Stability	13	75	12	100	±.00	104
Status Decline	13	70	17	100	−.07	72

Table 5. Change in expected number of children by change in economic status, 1955–1958: Re-interview Study.

tional scheme (upper and lower blue collar, upper and lower white collar) or an increase of greater than $1,500 in the husband's income during the three-year period. Status decline is defined as current unemployment, an occupational change involving a downward shift, or a decline in income.[15] The results of Table 5 certainly do not agree with the impression one would derive from answers given by the respondents. An improvement in status is more likely to produce a change in expectations than a decline, and the relationship, though weak, suggests that economic improvement results in a greater proportion of upward revisions than decline results in downward revisions. In general, the re-interview data suggest that although socioeconomic changes produce some revisions of fertility estimates, the primary determinants of the accuracy of fertility predictions lie in the family cycle characteristics.

The 1958 Study

The final source of data used in the evaluation of family size expectations is derived from a cross-section sample of greater Detroit adults done at the same time as the re-interview study. In the 1955 sample, only married females were eligible as respondents; in this sample, a randomly selected adult from each

[15] Median income increased by approximately $900 in Detroit during this period. *See:* The Detroit Area Study: Family Income in Greater Detroit: 1951–1957. 1958 (mimeographed).

sampled dwelling unit was interviewed. In both instances, the identical set of fertility questions was used. The data from the 1958 study, which are reported here, refer only to married persons who are less than 40 years old.

It should be recalled that the 1955 sample of women under 40 had an average of 2.09 children and expected to have an average of 2.89. By 1958, the same age group had given birth to 2.23 children, expected 3.21. Clearly, the slump in Detroit had not produced a decline in family size expectations, but why the increase? Only part of this increase can be explained by the difference in the marriage cohorts represented in the two samples. In 1955, 20 per cent of the sample consisted of prewar marriages whose eventual completed fertility will be lower than the post-war marriages. By 1958, 10 per cent of the sample were married in the prewar period.

Table 6 shows expected number of children classified by various characteristics of the 1955 and 1958 samples. The increase in expectations of .32 for the total population is not to be accounted for by the presence of males in the 1958 sample. Men and women have about the same expectations. We find a fairly uniform increase in expectations among socioeconomic groupings. However, there is some indication that the higher status groups have increased their family size expectations to a greater extent than the lower status groups over the three year period in question. This may reflect a continuation of the contraction in fertility differentials.

The major change in expectations is located among the religious groups. The expectations of Protestants have remained fairly stable at about three children per married couple, whereas Catholic expectations have increased by an average of .70 between 1955 and 1958. These data are in marked contrast to the results of the re-interview study, which showed that changes in expectations were negligible for both Protestants and Catholics. We find it difficult to believe that such a change could have occurred in the true value of expectations. Instead, we would like to suggest that the change in expected number of children

among the Catholics reflects a difference in the general content of the 1955 and 1958 interview schedules.

Although the fertility questions were the same in both studies, the 1955 research focused on nuclear family and kinship rela-

Table 6. Mean expected number of children by selected characteristics: 1955 and 1958 studies.

Selected Characteristics	Mean Expected Number of Children, 1955 and 1958				
	1955	1958	1958 Less 1955	Number of Cases	
				1955	1958
Sex					
Male	—	3.18	—	—	109
Female	2.89	3.24	+.35	427	118
Family Head's Occupation					
Blue Collar	2.93	3.20	+.27	270	134
White Collar	2.82	3.22	+.40	157	91
Family Head's Income					
Under $5,000	2.94	3.19	+.25	212	62
$5,000 and Over	2.83	3.22	+.39	205	160
Wife's Education					
Less Than 12 Years	3.03	3.30	+.27	186	75
12 Years and Over	2.78	3.17	+.39	241	152
Race					
Non-White	3.32	3.77	+.45	77	37
White	2.79	3.10	+.31	350	190
Wife's Farm Experience					
Some	3.05	3.38	+.33	115	45
None	2.83	3.16	+.33	311	182
Age in Years					
20–29	3.13	3.41	+.28	178	94
30–39	2.71	3.07	+.36	249	133
Wife's Religion					
Catholic	2.95	3.65	+.70	164	78
Protestant	2.91	3.00	+.09	233	132
Other	2.32	2.82	+.50	30	17
Total	2.89	3.21	+.32	427	227

tionships, while the 1958 study dealt primarily with religious behavior. Respondents were asked about their religious preference, participation, beliefs, values, and commitments. Some thirty minutes of questions reminding the respondent of his position within his own church could have affected the family size expectations of the Catholic respondents. In this case, it would appear that the content of the study had the effect of producing a loaded question on fertility expectations for our Catholic respondents. Apparently the impact of the religious questions was more sharply felt by the female Catholics whose average expectations were 3.83 children in contrast to the 3.53 children expected by the male Catholics. There was practically no difference in the responses of the male and female Protestants (3.03 and 2.98).

If Catholics are excluded from the analysis because of a presumed bias of their answers in 1958, the net change in expected number of children among non-Catholics in the 1955 and 1958 studies is from 2.85 to 2.98, only slightly more than could be expected on the basis of the change in marriage cohorts reflected in the two samples, and well within the sampling error. In this case we may say that the 1955–1958 sample comparison is basically in agreement with the results of the re-interview study in showing stability of family size preferences.

Summary

One factor clearly emerges from all of the data used in this paper—a short period of recession does not appreciably alter the average expected family size responses. Most persons probably view the slump as a temporary phenomenon. Although changes in the timing of births respond to the fluctuations in economic conditions, it would probably require a much more extended decline in the economy before completed family size expectations are revised downward.

Since our telephone re-interview data show practically no net change in expected number of children we should expect that cohort projections will be relatively accurate as long as

the cohorts upon which the expectations are based contribute the major share of the births in any given period. However, neither cohort nor any other type of projection can hope to deal with short term fluctuations in births.

At the individual level, it seems quite possible that as many as two-thirds of the newly married couples may change their family size preferences before passing completely through the reproductive period. We do not feel that these changes make expected number of children a poor variable for the analysis of social and psychological factors affecting fertility. If women change their expectations because of fecundity impairments, accidents, or through some unanticipated shift in the status of the family, it is precisely these factors which one would want to eliminate in studies of differential fertility. From the standpoint of the analyst, accidents and fecundity impairment represent error in the dependent variable. Obviously expected family size cannot be used as a substitute for completed family size at the individual level, but the preferences themselves may offer a conceptually purer and methodologically more useful variable in fertility studies.

BIRTH AND DEATH REGISTRATION IN MASSACHUSETTS

IV. THE SYSTEM ATTAINS ITS BASIC GOALS, 1870–1900*

ROBERT GUTMAN**

IN THREE earlier papers, I have tried to describe the development of the birth and death registration system of Massachusetts between 1639 and 1869, the date when the Board of Health was established. The present paper, the last of the series, is concerned with the history of the system between 1870 and 1900. In those thirty years, the basic goals of registration were finally attained. Birth registration became virtually complete by the end of the nineteenth century.[1] The returns of the causes of death achieved a degree of accuracy that was commendable, given the relatively low standards of medical training which still prevailed. The quality of the annual Registration Reports was good enough to meet the needs of most users. Important measures were adopted to increase the frequency with which the returns of mortality and the causes of death were made available for the work of sanitarians and local boards of health. The critics who proposed reforms in the registration system during the 1850's and 1860's had been thwarted in their hope that the State Board of Health would replace the department of the Secretary of State as the agency primarily responsible for registration.[2] But because the General Court allowed the Board to make a liberal interpretation of the powers granted to it for investigating the public health,

* I wish to thank the Population Council, Inc. whose generous support enabled me to conduct the research on which this paper is based.

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[1] Death registration was almost complete by 1870. *See* Gutman, Robert: Birth and Death Registration in Massachusetts: III. The System Achieves a Form, 1849–1869. Milbank Memorial Fund *Quarterly*, July, 1959, XXXVII, No. 3, p. 310; and Gutman, Robert: THE ACCURACY OF VITAL STATISTICS IN MASSACHUSETTS, 1842–1901. Ann Arbor: The University of Michigan Microfilm Series, 1956, p. 231 and *passim*.

[2] Gutman, Robert: Birth and Death Registration in Massachusetts: III. The System Achieves a Form, 1849–1869. *loc. cit.* p. 325.

the Board and the Secretary of State were able to work out an effective administration of the birth and death registration system.[3]

Registration of the Causes of Death

Although the registration of the causes of death was more accurate in 1870 than it had been in the decade preceding the formation of the State Board of Health, it was nevertheless true that 5 per cent of the returns for 1870 had no cause of death assigned to them. Perhaps even more distressing was the fact that a significant proportion of the deaths for which causes were assigned used outmoded or relatively meaningless nosological terms, such as "teething," "old age" and "infantile diseases" (Table 1). Several factors contributed to the unfortunate condition of the records. Clerks and undertakers did not use the powers granted them under the law of 1860, to request physicians to provide a certificate of the cause of death of persons whom they had attended. Indeed, an inquiry which the State Board of Health conducted in 1876 revealed that physicians in many of the towns never had been asked to fill out a medical certificate, in spite of the fact that the law was passed seventeen years before this date.[4] Many physicians who were approached by clerks and undertakers refused to comply with the law because they were given no fee for writing a certificate.[5] The physicians complained, furthermore, that the law did not discriminate between certificates written by members of the Massachusetts Medical Society and those which were made by amateur empirics and quacks.[6] Since undertakers had the power

[3] The powers of the Board are described in the bill which established it. *See* Massachusetts, First Annual Report of the State Board of Health. Boston: Wright and Potter, 1870, pp. 7–8. (In subsequent references this series will be listed as First State Health Report, Second State Health Report, and so on.)

[4] Massachusetts: Eighth State Health Report, pp. 235–248, *passim*.

[5] *Ibid.*, pp. 251–258, *passim*. Also *see* Buckingham, Charles E.: False Certificates of Death and the Registration Law. *Boston Medical and Surgical Journal*, 1868, 1, p. 225; and Derby, George: The Registration of Deaths. *Boston Medical and Surgical Journal*, 1868, 1, p. 265.

[6] Buckingham, *op. cit.*, 1, p. 225.

Year	Cause of Death										
	Cholera Infantum	Debility	Infantile Diseases	Old Age	Teething	Heart Disease	Consumption	Cancer	Peritonitis	Stillborn	Unknown
1870	6.5	1.2	1.9	6.4	1.2	3.6	18.9	1.9	0.2	2.4	4.7
1871	5.4	1.7	2.2	6.3	1.0	4.0	18.0	2.0	0.1	3.8	4.6
1872	9.3	1.6	2.1	5.6	1.2	3.4	16.3	1.6	0.1	2.6	4.4
1873	7.4	1.9	—	5.7	1.1	3.8	16.7	1.8	0.2	2.8	4.1
1874	6.8	2.0	—	5.5	1.0	3.9	16.4	1.8	0.2	3.4	3.6
1875	7.3	1.9	—	5.6	1.0	3.8	16.8	1.6	0.3	3.2	2.9
1876	6.1	2.4	—	5.7	0.9	3.8	16.3	1.9	0.3	3.1	2.9
1877	5.6	2.2	—	5.6	0.9	4.4	17.3	1.8	0.3	3.2	3.0
1878	4.9	2.5	1.7	5.9	0.7	4.5	16.6	2.5	0.5	3.4	2.6
1879	3.9	2.3	1.1	5.6	0.4	4.6	16.2	2.7	0.7	3.2	2.3
1880	6.0	2.5	1.1	5.5	0.5	4.8	15.4	2.5	0.7	3.2	1.8
1881	5.3	2.8	1.2	6.0	0.5	5.6	16.2	2.6	0.8	3.4	1.7
1882	6.1	2.6	1.8	6.5	0.4	5.5	15.8	2.6	0.8	3.4	1.4
1883	5.0	3.1	1.5	6.3	0.1	5.9	16.0	2.6	0.9	3.9	1.6
1884	5.7	2.8	1.8	6.3	0.3	5.9	15.7	2.7	0.8	4.1	1.1
1885	4.9	2.4	1.6	6.2	0.3	5.9	15.6	2.8	0.9	3.8	1.0
1886	5.3	3.0	2.0	6.0	0.3	6.9	15.1	2.8	0.9	4.4	0.9
1887	5.4	3.2	2.0	5.6	0.3	7.5	14.1	2.7	0.9	4.2	1.6
1888	5.2	2.6	1.9	5.4	0.2	7.2	12.5	2.9	0.9	4.1	1.5
1889	5.4	3.5	2.2	5.1	0.2	8.3	12.7	3.2	1.2	4.6	1.5
1890	6.1	3.6	—	4.6	0.2	8.0	13.1	3.2	1.2	4.6	—
1891	6.9	3.8	—	4.8	0.2	9.6	12.1	3.2	1.1	4.7	—
1892	6.3	3.3	—	4.4	0.2	8.1	11.6	2.9	1.2	4.5	—
1893	5.2	2.4	2.4	3.6	.09	7.2	10.7	3.0	1.1	4.7	1.1
1894	5.4	2.5	2.4	3.4	.12	7.4	11.1	3.2	1.2	4.8	1.0
1895	4.8	2.4	2.3	3.4	.15	7.6	11.0	3.5	1.4	4.7	.9
1896	6.0	2.9	2.7	3.5	.14	7.8	11.2	3.6	1.2	5.3	1.0
1897	4.7	2.7	2.9	3.4	.12	8.1	11.5	3.7	1.1	5.6	.8
1898	5.0	2.7	3.0	3.6	.11	8.2	11.3	4.1	1.2	5.8	.7
1899	4.1	2.7	3.1	3.6	.08	8.3	10.9	3.9	1.1	5.6	.7
1900	4.7	2.1	3.4	3.3	.14	8.0	10.2	3.9	1.1	5.6	.5

Source: Twenty-Ninth Registration Report to Fifty-Ninth Registration Report inclusive.

Table 1. Ratio of selected causes of deaths to total deaths in Massachusetts, excluding Boston, 1870 to 1900 inclusive.

to request certificates but were not required to do so, one obvious solution to the faulty operation of the system would have been to pass an amendment to the registration law requiring them to request a certificate. But this step was not taken until the very end of the period we are considering, in 1897,[7] perhaps because it was assumed that most of the physi-

[7] Massachusetts, Acts and Resolves Passed by the General Court for the Year 1897. Boston: Wright and Potter, 1897, chap. 444. (In subsequent references this series will be listed as Acts and Resolves.)

cians would not have complied with it. Another solution would have been to offer the physicians a fee; but this idea, too, was ignored, a fact which probably reflected the low prestige of the medical profession among members of the General Court and the public at large.[8]

It is worth noting one general characteristic of the measures which were eventually taken to close the gaps in the system for recording the causes of death. The great majority of them applied to the operation of registration in the towns and cities, rather than on the level of the administration of the system by the officials of the State government in Boston. When reforms of registration were advocated during the 1850's and 1860's by the Massachusetts Medical Society and other professional organizations, it was believed that little improvement could be achieved by changing the laws as they affected the performance of undertakers and clerks. It was the feeling at the time that the major focus of change should be the office of the Secretary of State. If expert medical personnel, instead of laymen in the Secretary's office, could somehow be made responsible for registration, so the argument ran, all the problems of registration would clear up by themselves.[9] Two events, or series of events, ultimately disabused the critics of registration of this idea. In the first place, by establishing a State Board of Health, and then not giving the Board direct control over registration, the General Court indicated unmistakably its wish to keep registration in the hands of the department of the Secretary of State. And in the second place, such reforms as the Secretary had introduced into his supervision of registration did not eliminate the poor quality cause of death returns.

[8] For a discussion of the low prestige of the medical profession during the nineteenth century, *see* Gutman, Robert: Birth and Death Registration in Massachusetts: II. The Inauguration of A Modern System, 1800–1849. The Milbank Memorial Fund *Quarterly*, October, 1958, XXXVI, No. 4, p. 381. Incidentally, it was just about this time that the prestige of the medical profession began to turn upward, as a result of the great advances in scientific medicine made during the last quarter of the nineteenth century. Massachusetts began to license physicians again in 1894.

[9] *See* Gutman, Robert: Birth and Death Registration in Massachusetts: III. The System Achieves A Final Form, 1849–1869, *loc. cit.*, p. 318.

In trying to improve registration by reforming the system as it worked on the local level, the General Court enacted an amendment to the registration law in 1872. This law ordered all towns and cities in the State to license undertakers, who were then threatened with the loss of a license if they failed to perform the duties required by law, including the duty of reporting deaths.[10] All the cities in the State had licensed undertakers, under powers granted by the Health Act of 1849.[11] Now all the towns as well as cities were required to license them. Unfortunately, compliance with the law was limited: an examination of town financial reports reveals that probably no more than one-fourth of the towns which had not licensed undertakers previously did so between 1872 and 1875.[12]

The law was passed at the behest of the State Board of Health, which decided at its January, 1872, meeting, to address a petition about the subject of undertakers to the General Court. The action of the Board was precipitated not only by its knowledge of the weaknesses of the cause of death data, but more immediately, grew out of the accumulating evidence that the health conditions of the State were becoming worse, especially in the urban areas. The Board hoped that the licensing of undertakers would reduce the number of sanitary nuisances brought about by poorly prepared burial sites and inadequately embalmed bodies.[13]

[10] The text of the law is given in Massachusetts, Acts and Resolves, 1872, chap. 275. For the legislative history of the bill, *see* Massachusetts, *Journal of the House of Representatives of the General Court of the Commonwealth*, 1872, p. 419ff. and *Journal of the Senate of the General Court of the Commonwealth*, 1872, p. 309ff. (These sources will be referred to hereafter as *House Journal* and *Senate Journal* respectively.)

[11] *See* Massachusetts, Acts and Resolves, 1849, chap. 211. The fact that the various cities took advantage of their powers under the law is known from an examination of their ordinances and municipal registers. The best collection of these sources is available at the Massachusetts State Library, State House, Boston, Mass.

[12] The financial reports of the towns are also available at the Massachusetts State Library.

[13] Records of the Massachusetts State Board of Health, I, 1869–1879, p. 139. This is a bound manuscript volume preserved in the office of the Commissioner of Health, State House, Boston. The petition is included along with the manuscript copy of the bill, on file in the Massachusetts State Archives, State House, Boston.

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Two additional amendments were incorporated in the registration law in 1873. One amendment continued the tradition begun in 1860 of offering extra rewards to clerks who *collected* deaths in addition to having recorded them. The fee paid the clerk who performed both these tasks was raised from twenty to thirty-five cents.[14] The other amendment raised the fee of undertakers for returning a death from ten cents for each death to twenty-five cents.[15] Although neither of these additions to the registration laws was introduced at the direct suggestion of the State Board of Health, some members of the Board were consulted by the General Court when the bills were being considered. The amendments themselves appear to have been initiated by complaints addressed by the clerks and undertakers to their representatives in the legislature.[16]

The Legislation of 1878

A glance at Table 1 will show that only the proportions of deaths attributed to consumption were lower in 1872 than before, and also continued at this new level thereafter. Even if we ascribe the decline in the proportion of deaths of unknown cause which became marked in 1874 to the new legislation, it is obvious that the amendments of 1872 and 1873 had a very limited impact on the accuracy of the returns of the causes of death. Dissatisfaction with the causes of death registration continued, and several attempts were made subsequently in the 1870's, and in the 1880's, to plug the loopholes in the system. The most important attempt was a many-faceted amendment enacted in 1878. This amendment provided, first, that neither a burial was

For a discussion of the worsening health conditions in urban areas following the arrivals of heavy tides of immigration, *see* Handlin, Oscar: Boston's Immigrants, 1790–1865. Cambridge: Harvard University Press, 1941, pp. 117–119. These conditions were reflected in the rising death rates recorded in the annual Registration Reports. *See* Gutman, Robert: The Accuracy of Vital Statistics in Massachusetts . . ., pp. 249–258.

[14] Massachusetts, Thirty-First Report Relating to the Registry and Return of Births, Marriages and Deaths. Boston: Wright and Potter, 1874, p. clvi. (These reports will be referred to hereafter as Thirty-First Registration Report, Thirty-Second Registration Report, and so on.)

[15] *Ibid.*, p. clvi.

[16] Massachusetts, *House Journal*, 1873, pp. 162 ff. and pp. 471 ff.

to take place in a community nor was a body to be removed from it, without the record of death having been returned to the clerk.[17] Previous legislation, it will be recalled, had provided that the undertaker obtain a burial permit before the burial, *when practicable.* The latter qualification was stripped from the new version. Secondly, the law provided that "no clerk or local registrar shall give such certificate or burial permit, until the certificate of the cause of death from the physician, if any, in attendance at the last sickness of the deceased has been obtained, and placed in the hands of said clerk or local registrar."[18] By means of this provision, the law apparently intended to force undertakers to request certificates from physicians, although the law did not state this demand explicitly. Finally, the law provided that, in cities and towns where boards of health had been established, the local boards be required to approve all certificates of the cause of death prior to the grant of a burial permit.[19] The last provision of the law is especially interesting because it is indicative of a shift in strategy after 1869 among the reformers of the registration system. These critics, of whom the members of the Massachusetts Medical Society were probably the most notable, continued to believe that it was necessary to bring medical personnel into a more direct supervisory relation over the registration system. But with the General Court apparently unwilling to give the Board of Health control of the state administration of registration, the only remaining alternative was to make use

[17] The text of the law is given in Massachusetts: Thirty-Seventh Registration Report, p. clxv; also in Massachusetts: Acts and Resolves, 1878, chap. 174. Portions of the legislative history of the bill can be found in Massachusetts, Legislative Documents of the Senate of the General Court of the Commonwealth, 1878, No. 20 and No. 70. (This source will be referred to hereafter as Senate Documents, and its companion series relating to the House of Representatives of the General Court as House Documents.)

[18] Massachusetts, Thirty-Seventh Registration Report, p. clxv.

[19] *Ibid.*, p. clxv. The bill also provided that in cases of violent deaths, the medical examiner should be the person who would furnish the required certificate of the cause of death. This provision followed logically from a law passed in 1877 which abolished the office of coroner and thereby separated the medical and legal responsibility involved in the investigation of violent deaths. The provision was intended to add force to the law requiring burial permits by making it more difficult for deaths caused by homicide or suicide to go unregistered.

of the organizations of medical personnel on the level of local government. The idea was first developed in Boston where, in 1874, the Boston Medical Society had tried to get the Mayor and Council of that city to abolish the office of City Registrar and substitute for it an office of registrar of death and burials under the authority of the Boston Board of Health. But because such reorganization of the Boston registration system would have been inconsistent with the requirements of the State registration law, the Boston Medical Society withdrew its proposal. Instead the members of the Society pressed for a revision of the State laws, which, with the support of the State Board of Health, then eventuated in the law of 1878.[20]

In spite of obstacles to its enforcement, the law of 1878 did improve the accuracy of the returns of the causes of death (Table 1). For instance, the proportion of deaths assigned to "unknown" causes declined from 3 per cent in 1877 to 2.3 per cent in 1879 and beginning with 1880 never rose above 1.8 per cent. (At this date, the category "unknown" included returns lacking any information as well as returns using such vague terms as "tumor" or "hemorrhage.") Teething as a cause also declined significantly beginning in 1879 although there was no marked change downward in the proportion of deaths assigned to some of the other ambiguous diseases. Perhaps the most convincing evidence of the ameliorative impact of the new law was the increase in the percentage of deaths said to have been caused by cancer, peritonitis, and heart disease. These were causes of death that trained medical practitioners were likely to assign but which were relatively unfamiliar to laymen. In other words, the rise in deaths ascribed to these diseases would be consistent with a view that, as a result of the 1878 law, more deaths were returned to the clerks

[20] *See* Boston: SECOND ANNUAL REPORT OF THE BOSTON BOARD OF HEALTH. Boston: Wright and Potter, 1874. Also Boston: *Majority and Minority Reports on the Duties of the City Registrar and Board of Health* (Boston City Documents, 1875, No. 70); and Boston, *Reply of the City Registrar to the Charge of Inaccuracies in His Department* (Boston City Documents, 1876, No. 36). The State Board of Health's opinion of the bill is given in Massachusetts: NINTH STATE HEALTH REPORT, p. xiii.

along with medical certificates from physicians than had been the case earlier in the history of registration. In addition to increasing the participation of physicians, the law seems also to have brought about greater cooperation from the undertakers, especially from the undertakers in small towns. In many such communities undertakers had made no returns of death before 1878 but then began to comply with the law either in that year or shortly thereafter.[21]

I have mentioned that there were certain obstacles to the enforcement of the law. One obstacle was that relatively few Massachusetts communities in 1878 had local boards of health which were separate from the town selectmen and which included a physician or other person familiar with medical matters.[22] According to a law passed in 1836, all towns in the State were given the right to appoint such boards, and failing that, the selectmen were automatically constituted the board. But apparently few towns made use of their appointive powers.[23] The situation was more complicated in the cities, because unlike the towns, the city governments did not have the right to appoint boards of health unless they received specific permission to do so from the General Court. Only Boston had asked for this permission, which was granted in 1872.[24] To put it in another way, the provision of the 1878 law that local boards of health approve medical certificates before burial was con-

[21] For the effect of the 1878 law on registration practices in small towns, *see* the financial reports for the period 1875–1885 of the towns of Bridgewater, Hinsdale, Norton, and Westport, Massachusetts.

[22] Of the more than three hundred towns in Massachusetts, 185 replied to a survey dealing with local boards of health which the State Board conducted in 1875. Of the 185 communities, the selectmen acted as the board in 80, there were no boards in 70 (which probably was equivalent to saying that the selectmen acted as such) and in only 26 towns were there separate boards of health. It is likely that there also were no separate boards in the towns which failed to reply to the inquiry. Massachusetts: SEVENTH STATE HEALTH REPORT, p. 527.

[23] Massachusetts: THE REVISED STATUTES OF THE COMMONWEALTH OF MASSACHUSETTS. Boston: Dutton and Wentworth, 1836, p. 208. Also *see*, Massachusetts, Commissioners on the Sanitary Survey: REPORT OF A GENERAL PLAN FOR THE PROMOTION OF PUBLIC AND PERSONAL HEALTH. Boston: Dutton and Wentworth, 1850, p. 52 ff.

[24] Boston: *Report of the Joint Special Committee on Intramural Interments.* (Boston City Documents, 1879, No. 96).

structed on a legal fiction. Fortunately, this fact was immediately obvious to the State Board of Health which then undertook a campaign to get towns to use their powers and also urged cities to seek the right to appoint boards. As a result of this campaign, all the cities of the State had separate boards of health by 1888, and boards of health also were established in about sixty per cent of the towns in the State by the same date.[25]

Another obstacle to the enforcement of the law was the extra labor it imposed on undertakers. Any undertaker who wished to fulfill his duties with respect to registration before 1878 had to do two things: he had to obtain a certificate of the cause of death from a physician and he had to bring this certificate to the town clerk, along with other information. Even if he was as scrupulous as the old law intended, and more scrupulous than it required, and he brought the information to the clerk before the burial, he still had to deal with only two people. But according to the new law, he had now to perform at least three tasks: he had to get the physician's certificate, he had to bring it to the local board of health for their approval, and then he was supposed to carry it to the town clerk's office. And all this within a matter of hours or a few days at the most, or else the body could not be buried. The amount of extra labor imposed by the third task is perhaps fully understood only when we realize that very few communities, indeed less than ten per cent in the whole State, had an office where the clerk could be found regularly; and in even fewer towns and cities was there a place where the board of health met.[26] Undertakers in Boston were so irate over the conditions imposed by the new law that they organized their colleagues in every town and city in the State in order to address a common petition to the General Court calling for some reform of the law. In their petition, the undertakers were joined by a group of Boston physicians. The petition was submitted to the General Court in the Spring of

[25] Massachusetts: Twentieth State Health Report, p. 265.
[26] Massachusetts: Eighth Report of the Commissioner of Public Records. Boston: Wright and Potter, 1896, p. 5.

1880. It called for a reorganization of the system to give boards of health the power not only of approving the certificate of the cause of death but also of granting the burial permits.[27] The petition was referred to the Committee on Public Health, but the Committee took no action on it.[28] In the following year, the undertakers recommended an alternative solution, namely that the old arrangement be reinstituted and that the clerk be given the right to approve the certificate as well as to grant the burial permit. This recommendation, too, went unheeded.[29] The undertakers probably would have had more success were it not that the State Board of Health, generally the great rational force in matters affecting registration, was merged with the Boards of Lunacy and Charity in 1879. As a consequence, the powers, even the advisory powers, of the Board in legislation affecting health and statistical questions was severely limited.[30] Indeed, not a single improvement was made in the registration law between 1879 and 1886, when the Board of Health again was granted independent status.[31] On

[27] The petition, from which the activities of the undertakers and physicians can be inferred, is included in the manuscript version of the order presented to the General Court in 1880, on file in the Massachusetts State Archives, State House, Boston.

[28] Massachusetts, *Senate Journal*, 1880, pp. 21, 225 and 233.

[29] *Ibid.*, 1881, pp. 84 and 291.

[30] Henry Bowditch, who was appointed the first chairman of the State Board of Health in 1869, wrote of the merger of the three boards as follows: "In 1878 came mutterings of political disaster to the ruling powers, and forebodings of what the renowned Gen. Butler [Benjamin "The Beast" Butler, Civil War general, Congressman from Massachusetts, and later Governor] would do with the numerous 'commissions' (that of health among them) that were 'spending wastefully the people's money.' Accordingly, to attack this redoubtable general upon his political 'flank,' the legislature, under suggestions from Governor Talbot, merged the three departments of health, lunacy and charity, a Cerebus, in fact, in its grotesqueness of head. Three commissions, all different in ideas and modes of action, jumbled into one heterogeneous mass, simply because the ruling party feared the advent to power of a political adventurer! The prospects were chilling in the extreme to me, and I soon found two sad results: viz., heartburning and jealousies among the increased number of members, and an almost total neglect of sanitary work. At one time, for three or four successive meetings, nothing was done about sanitation, . . . Such neglect of that which we had been for years laboring for was distressing." Bowditch, Vincent Y.: Life and Correspondence of Henry Ingersoll Bowditch. Boston: Houghton Mifflin, 1902, II, pp. 226–227.

[31] MacDonald, Eleanor J.: A History of the Massachusetts Department of Public Health. *The Commonhealth*, 23 (1936), p. 98.

the recommendation of the reconstituted Board, and with the help of a petition sent by the Mayor of Boston on behalf of the Boston Board of Health, the General Court in 1888 took up the problem imposed on the undertakers by the registration law and finally granted local boards of health the right to grant burial permits.[32]

It is testimony to the achievement of the system for registering causes of death during the period between 1870 and 1888 that no further innovation in its operations was introduced during the remainder of the nineteenth century.

Improvements in the Registration Reports

In earlier papers in this series, attention was called to the fact that the motivations for recording vital events in the seventeenth, eighteenth, and also in the early nineteenth century were almost exclusively legal and genealogical. By the time a modern statistics system was established in Massachusetts in 1840, this motivation had undergone a change. The State government of Massachusetts was led to assemble vital records by the appeals of the new professional statistical organizations, and by the medical societies and allied public health groups. What mainly interested these groups were records of death, especially accurate records of the causes of death. They hoped to use these data to study the factors associated with disease and to forewarn physicians and their patients of the approach of epidemics. It was in order to facilitate the fulfillment of these ambitions that so much effort was applied to getting undertakers to secure physicians' certificates. Partial success along these lines was not sufficient, however, if the goals of the consumers of registration data were to be achieved. Even as the accuracy of the records returned by undertakers and clerks

[32] In towns in which there were no separate boards of health, the responsibility for granting burial permits remained with the town clerks. Massachusetts: Acts and Resolves, 1888, chap. 306. For the events leading up to the enactment of the law, *see Boston City Documents*, 1888, No. 17; Boston, *City Council Minutes*, 1888, pp. 34–36; Records of the Massachusetts State Board of Health, III, p. 44 (this is a bound manuscript volume preserved in the office of the Commissioner of Health, State House, Boston); and Massachusetts, *House Journal*, 1888, p. 80.

improved, there was still the question whether the data would be analyzed and displayed properly in the annual Registration Reports which the Secretary of State submitted once each year to the General Court. And, furthermore, there was the problem of making the data available to consumers more immediately following their collection. Physicians, local boards of health, and sanitary organizations needed the information quickly in order to combat epidemics. The State Board of Health, making use of its general powers to inquire into the condition of the public health and to cooperate with other agencies of the government in matters related to health, did something about both of these problems between 1870 and 1900.

The work of the State Board of Health with the annual Registration Reports began when the Secretary of State inquired whether the members of the Board would be interested in having their Secretary edit the Reports. The Secretary of State believed that it would be more economical for the government if the Board's Secretary would perform the work as part of his regular salaried duties.[33] The members of the Board accepted the opportunity because they saw in it a chance to achieve by administrative fiat one of the central goals of the public health movement.[34] The first fruits of the liaison appeared in September of 1879, when the Registration Report for 1878 was published under the editorship of Dr. Charles Folsom, Secretary of the State Board of Health.

The Registration Reports for 1869 through 1877 had continued to exhibit the improvement in form which had characterized their development during the Civil War years and immediately thereafter. One example of the improvement was the order and consistency introduced into the supplementary tables included in the text of each volume. The tables were chosen to illustrate particular aspects of the returns which the editor of the Report wished to discuss. Some of these tables,

[33] Massachusetts: First Report of the Board of Health, Lunacy and Charity. Boston: Wright and Potter, 1879, p. xlcv. (This series will be referred to hereafter as Health, Lunacy, and Charity Report.)

[34] Massachusetts: Tenth State Health Report, p. xxxix.

such as those showing the variation in fertility and mortality by nativity, were indispensable to the student of vital trends and public health, although they were not part of the basic tables of the Reports. One of the weaknesses of the Reports before 1869, however, was that these supplementary tables tended to appear or disappear rather haphazardly, depending on the editor's whim. After the Board of Health was established, a feature of the reports was that the same supplementary tables were included in each successive volume.[35]

When Folsom took charge of the reports several additional changes were made. For many years prior to 1878, a table entitled "Comparative Mortality," showing the causes of death for the sum of all deaths occurring since 1842, nosologically classified, and for each of the previous five years separately for each year, had been included among the basic tables.[36] Folsom introduced the innovation of reducing the number of years covered by the table to those beginning with 1857, and of showing the causes of death in *each one* of the intervening years in a separate column. Another innovation, of even greater utility, was a table which classified several leading causes of death *by towns* for the year covered by the report.[37]

Another change adopted in 1878 occurred in the basic table which abstracted the fertility and mortality returns of the previous decade. In 1877, and in earlier years, this table had employed the ratio of deaths to population as an index of the trend of vital events.[38] Folsom substituted for it the average age of the decedents in each year. He was, of course, aware of the serious deficiencies of this measure as an index of mortality, but he believed that, when used comparatively, it was more reliable than a crude death rate based on an extrapolated esti-

[35] Some examples of the most important of the supplementary tables, in addition to those showing births and deaths by nativity, were tables of births by quarters of the year; the sex ratio of births; mortality by geographical divisions; deaths by quarters of the year and the order of the ten principal causes of death. *See* Massachusetts: Thirty-First Registration Report, pp. 5, 6, 23, 25 and 48.

[36] Massachusetts: Thirty-Sixth Registration Report, pp. c–cvii.

[37] Massachusetts: Thirty-Seventh Registration Report, pp. c–cvii and cxvi–cxxv.

[38] Massachusetts: Thirty-Sixth Registration Report, pp. cxiv–cxxxv.

mate of population size for each year. Folsom based this view on the extreme volatility of the Massachusetts population resulting from the large annual shifts in the amount of immigration, and the difficulty of obtaining accurate data on the number of immigrants.[39] Folsom's scepticism about extrapolated population estimates was carried over into the supplementary tables included in the text. For instance, in the tables which in previous volumes had shown the trend of the birth and death rates since the inauguration of a modern statistics system, he printed ratios only for the census years; and for the other years, he simply listed the number of births and deaths in those years.[40] In the section of the text included in the Report for 1880, Folsom presented a number of special supplementary tables dealing with diphtheria and croup; in the report for 1881, a similar analysis was provided of deaths by typhoid fever.[41] For all three diseases, the number of deaths in each town in the State over a number of years was listed. To make the reports more helpful to the sanitarian, Folsom began the practice of numbering the supplementary tables. He also included a meteorological section, something which had not appeared since 1859.[42]

Folsom's work on the reports for 1880 and 1881 was done in a private capacity, as a consultant to the department of the Secretary of State. This arrangement came about because of merger of the State Board of Health with the Boards of Lunacy and Charity, after which event the agreement was revoked between the Secretary of State and the Board of Health to have the latter's Secretary edit the reports. The reports for 1882 to 1885 inclusive were edited by Dr. Frank Wells, a well-known vital statistician of the time and formerly health officer of Cleveland.[43] When merger of the three boards proved to be un-

[39] Massachusetts: Thirty-Seventh Registration Report, pp. iii–iv.

[40] *Ibid.*, pp. 9 and 38.

[41] Massachusetts: Thirty-Ninth Registration Report, pp. 86–110; Fortieth Registration Report, pp. 90–108.

[42] Massachusetts: Thirty-Seventh Registration Report, pp. 88–90.

[43] Massachusetts: Forty-First Registration Report, Secretary's Preface.

workable and an independent State Board of Health was set up again, the then Secretary of State renewed the request of one of his predecessors that the Secretary of the Board become the editor of the registration reports. The Board received the request with pleasure and Dr. Samuel Abbott became the editor of the report of 1886.[44]

The short space of time which elapsed between Abbott's appointment to the job and the date set for the publication of the 1886 report made it almost impossible for him to introduce any changes in its form or its contents. The report of 1887, however, which appeared in 1888, showed considerable evidence of his work. One table which had the causes of death arranged alphabetically by month, age, and sex of the decedent was replaced by two tables, one of which gave the causes of death arranged nosologically by month and sex and the other of which showed the same basic information, but by age and sex.[45] A new table listed the nativity of the parents of children, the nativity of the deceased, as well as the nativity of the parents of the deceased.[46] Tables similar to this particular one had been included among the supplementary tables of previous reports, but never among the basic tables. Abbott also changed the table which in earlier reports had given the occupation of deceased persons. In the revised version, it listed, in addition to this information, the occupation of fathers of children.[47] Abbott introduced innovations, too, in the section of the text dealing with mortality, including a supplementary table showing the death rate in relation to density of population, by county; another supplementary table listing the proportion of deaths returned with unspecified or unknown causes from 1865 onward; and a lengthy discussion of the returns made by the medical examiners.[48] In general, in his discussion of mortality, Ab-

[44] Massachusetts: Forty-Fifth Registration Report, Secretary's Preface. Also, Massachusetts, NINETEENTH STATE HEALTH REPORT, p. xlviii.
[45] Massachusetts: Forty-Fifth Registration Report, pp. lxxii-lxxxix; Forty-Sixth Registration Report, pp. 80–119.
[46] Massachusetts: Forty-Sixth Registration Report, pp. 170–171.
[47] *Ibid.*, pp. 172–179.
[48] *Ibid.*, pp. 326, 340 and 405–418.

bott carried out a more detailed analysis of specific causes of death than could be found in the registration reports edited by any physician or sanitarian before 1890.

The Weekly Returns of Mortality

The lag between the occurrence of deaths and the date when these events became known to health officials was a matter which concerned observers of the registration system almost since its foundation. Already in the 1840's, the members of the Massachusetts Medical Society, and the editors of the medical journals published in Boston, lodged protests with the Secretary of State and with the General Court over the delay of eighteen months or more between the collection of annual returns and the publication of the Registration Reports. These protests were effective and by the middle of the 1860's no more than one year, on the average, separated collection and publication.[49] But a reduction of the delay in the publication of the annual returns was only a partial solution at best, since the returns, after all, were filed with the Secretary of State only once a year. Something had to be done both to assemble and to issue the returns more frequently. The Medical Society did attempt during the 1850's to get the General Court to require the more frequent collection of returns from the towns as well as to have the Secretary of State initiate a system for the registration of diseases through regular reports from physicians in the State.[50] These suggestions were turned down by the State government and, for a while, the Society experimented with its own system for registering diseases, but with little success.[51] In the large urban communities, the problem was met by having

[49] An example of the complaints about the date of publication of the Registration Reports can be found in the *Boston Medical and Surgical Journal* for 1847, p. 539. After 1865, it was a favorite ploy of the Secretaries to write in their prefaces to the Reports that the corresponding document issued by the British Registrar-General was not published in England until two years after the date of the returns. *See*, for instance, the comment by Secretary of State Oliver Warner in Massachusetts, Twenty-Second Registration Report, p. vii.

[50] *See* Burrage, Walter L.: A History of the Massachusetts Medical Society. Boston: privately printed, 1923, pp. 137–138. Also *see* Massachusetts: Fifth Health, Lunacy and Charity Report, Supplement, p. 64.

[51] Burrage, *op. cit.*, p. 138.

the City Registrar or Clerk issue reports to the newspapers or medical magazines once a week. In Boston, for instance, such a system seems to have been set up soon after 1849.[52] But for the State as a whole, nothing was accomplished until the State Board of Health, making use of its general powers, decided at one of its first meetings in the fall of 1869 to collect returns of mortality and their causes each week and to publish them immediately.[53]

The Board sent forms and a circular announcing the plan and the reason for adopting it, to the clerks and registrars of the twenty most populous towns and cities of Massachusetts, and requested their cooperation. In view of the difficulty which previous efforts to introduce innovations in the field of vital registration had encountered, it was surprising and also encouraging that all of the communities expressed interest in the plan. By November of 1869, it was in full operation. The cooperation of clerks and registrars was facilitated by the fact that all these communities had an efficient system of burial supervision, which meant that undertakers made returns promptly and did not bury their dead without having first obtained burial permits. The returns from the twenty communities were published each Wednesday in the *Boston Morning Journal*.[54]

The weekly mortality returns were made to the Board more or less voluntarily, since there was no provision in the registration statutes which required towns and cities to file returns so often. It was not long, therefore, before members of the Board of Health were suggesting that the idea of the system be incorporated into the registration law, and that all the towns and cities in the State be required to file returns weekly.[55] The Board's recommendation was not adopted, however, at any time during the period covered by our study, although begin-

[52] *See* the series Boston: Report of the City Registrar of the Births, Marriages and Deaths in Boston for the Year. . . . The series begins in 1849.
[53] Massachusetts: First State Health Report, p. 14.
[54] *Ibid.*, pp. 17–18.
[55] Massachusetts: Eighth State Health Report, p. 15.

ning in 1894, towns and cities of 5,000 inhabitants or more were required by law to file returns with the Board annually. Nevertheless, through persuasion, the number of towns filing weekly returns was increased to over one hundred by 1883.[56] In fact, as time went on the Board discovered that it had a surfeit of data to deal with each week, so in 1886 it altered the system, and requested weekly returns only from towns of 10,000 population or over. All other towns were henceforth to file returns once a month.[57] Apart from the importance of these returns in helping the Board keep close tabs on the public health, they also undoubtedly served as a means of stimulating interest in the subject of registration in general, and probably help to explain why only one or two per cent of the deaths in Massachusetts as a whole went unregistered in 1900.[58]

Birth Registration

At the time of the formation of the State Board of Health the underregistration of births was a more serious problem than the underregistration of deaths. Only about ninety per cent of the births which occurred in 1870 were registered, compared to ninety-seven or ninety-eight per cent of the deaths.[59] In spite of this fact, when so much was done during the ensuing decade to improve the accuracy of death registration, almost no attention was paid to the condition of the system for registering births. One reason for this neglect was that it was more difficult to cope with deficiencies in the registration of births than of deaths. Large proportions of births—in rural areas, perhaps as many as fifty per cent—took place without either physicians or

[56] Massachusetts: Fifth Health, Lunacy and Charity Report, Supplement, p. 65.

[57] Massachusetts: Eighteenth State Health Report, p. xviii.

[58] *See* Gutman, Robert: The Accuracy of Vital Statistics in Massachusetts . . ., p. 231; also Massachusetts: Thirtieth State Health Report, p. 812; and Fisher, Irving: Mortality Statistics of the United States Census. The Federal Census: Critical Essays by Members of the American Economic Association. "Publications of the American Economic Association," New Series, no. 2 (New York, 1899), p. 128.

[59] Gutman, Robert: The Birth Statistics of Massachusetts During the Nineteenth Century. *Population Studies*, 1956, x, p. 76. Also *see*, Gutman, Robert: The Accuracy of Vital Statistics in Massachusetts . . ., p. 231.

midwives in attendance.[60] On the other hand, by the 1870's, almost every corpse was buried either by an undertaker or a sexton. In this way, the fact of death acquired a public and legal character lacking in the case of births. Parents and householders still were required by law to report births but virtually no parent was aware of the requirement.[61] And because parenthood obviously is not a governmental office, there were great obstacles to making parents comply with the law. Perhaps they could have been prosecuted had there been professional organizations actively interested in birth registration making efforts comparable to the work of the Massachusetts Medical Society in behalf of death registration. But there was no society of demographers during this period and the American Statistical Association seems not to have had any special interest in the problem of fertility analysis. The bar associations, whose members ought to have been aware of the importance of adequate birth records, did not debate the subject at their meetings. The genealogists had a certain concern for birth data but their attention was focussed on historical rather than contemporary records and they did not press for reform.[62] Probably the most telling evidence of the lack of interest in birth registration is that the State Board of Health took no position on the subject until ten years after it was established. Only in February, 1879, shortly before the Board lost its independent status, did its members decide to circularize physicians, directing their attention to the law regarding births and requesting their assistance

[60] *See* Gutman, Robert: Birth and Death Registration in Massachusetts: II. The Inauguration of A Modern System, 1800–1849. *loc. cit.*, p. 385.

[61] *Boston Medical and Surgical Journal*, 1868, I, p. 226.

[62] In the 1890's the genealogists were active in behalf of reforming the registration law but the measures they were successful in introducing were unrelated to the use of vital records for statistical and medical purposes. The measures dealt, instead, with such matters as the facilities for storing records, the types of indelible inks and papers used in transcribing them and the methods of binding and indexing the returns. For a discussion of this aspect of the genealogists' work, *see* Massachusetts: Report to the Legislature of Massachusetts Made by the Commissioners Appointed under Resolve, Chapter 60, 1884, upon the Condition of the Records, Files, Papers and Documents in the Secretary's Department. Boston: Wright and Potter, 1885, *passim*. Also *see* Massachusetts: First Report of the Commissioner of Public Records. Boston: Wright and Potter, 1889, *passim*.

in enforcing it.[63] The widespread apathy with regard to birth statistics resulted from the fact that fertility was not regarded as a social problem during the nineteenth century. The one possible exception was the worry manifested among some of the older ethnic stocks in Massachusetts that their fertility was not keeping pace with the birth rate among the immigrant groups. Their fears were based on observations of family life in the slums inhabited by the foreign-born, and it was confirmed by the returns published in the Registration Reports.[64] But the higher fertility of the immigrants did not become an incentive for revision of the birth registration law because no one doubted that what the returns showed in this respect was accurate.

The first change of any importance made in the birth registration law after 1869 was enacted in 1880.[65] This law was constructed along the lines of the birth registration bill passed in 1865 which then had been repealed in 1866. It required physicians and midwives in every town and city except Boston to return once each month to the town clerks a list of the births they had attended. The precise source of the bill which became the 1880 law is not known.[66] In view of the discussion of the subject at a meeting of the State Board of Health in 1879, the proposal may well have originated with it. On the other hand, we know that the bill was presented to the General Court by a

[63] Records of the Massachusetts State Board of Health, I, 1869–1879, pp. 528–529. This is a bound manuscript volume preserved in the office of the Commissioner of Health, State House, Boston.

[64] *See* a report of a talk on the subject given before an agricultural society in Lowell by Dr. Nathan Allen, statistician and a member of the State Board of Charities: APPLETON'S ENCYCLOPEDIA, 1866, p. 479. The research on which Allen's talk is based is printed in full in Massachusetts: THIRD ANNUAL REPORT OF THE STATE BOARD OF CHARITIES. Boston: Wright and Potter, 1867, pp. 19–31. The popular anxiety over what was called the "growing degeneracy" of the native stock is evident even in this presumably "scientific" paper.

[65] The only other change in the birth registration law to occur between 1869 and 1880 was the amendment adopted in 1873, which raised the fee of clerks, who *collected* as well as recorded births, from twenty-five to fifty cents for each birth. *See* Massachusetts: Thirty-First Registration Report, p. clvi.

[66] The legislative history of the bill is recounted in Massachusetts, *House Journal*, 1880, pp. 96 ff. The text itself can be found in Massachusetts, Thirty-Ninth Registration Report, p. clxxvi.

member of the House from Taunton and therefore we can guess that it may have been suggested by the clerk of that City. A careful analysis of the returns made under the new law indicates that it had no effect on the completeness of birth registration. Even prior to the passage of the law, only about five per cent of all births in Massachusetts escaped registration. The number of registered births in 1880 increased to 44,217 from the 40,295 births recorded in 1879 but the rise can be accounted for in terms of a higher level of fertility. The higher fertility resulted from the economic revival following the depression of 1873–1879 which, in turn, drew large numbers of immigrants to Massachusetts from Europe and French Canada, led to a rise in the marriage rate and allowed for renewed fertility among those families which had postponed having children during the depression.[67]

The law was not successful largely because the physicians in the State refused to comply with its provisions. Their objections were similar to those they made attacking the law of 1860, which first required them to return a certificate of the cause of death. In the case of both laws, physicians felt that by not offering them a fee for making a return, the State failed to acknowledge that they were performing a professional service. The low public prestige of the medical profession, combined with the fact that the State refused to assume the responsibility for licensing physicians, made doctors especially sensitive to laws of this sort. The members of the Massachusetts Medical Society, in particular, viewed such legislation as an insult to their professional independence and integrity. Unlike the objections to the death certificate law, however, medical opposition to the 1880 law was well organized. In 1882, the Medical Society discussed its provisions and voted to address a petition on the subject to the General Court. In their petition, delivered in 1883, the Society recommended that the towns and cities pay physicians and midwives twenty-five cents for each birth they

[67] Gutman, Robert: The Birth Statistics of Massachusetts During the Nineteenth Century, *loc. cit.*, p. 74.

certified.[68] A similar petition had been addressed to the legislature in 1882 by thirty-six physicians, all members of the Massachusetts Medical Society but who practiced in Berkshire County.[69] At the same time that these requests were addressed to the General Court, other physicians, also from the western part of the State, were recommending the wholesale repeal of the law and asking for a hearing on the subject.[70] All the petitions were referred to the Judiciary Committee. The opinion which held that the law should be reaffirmed and a fee provided for physicians and midwives prevailed in the legislature. A debate lasting several days developed, however, over the appropriate amount the physicians ought to receive. A fee of twenty-five cents finally was established and the bill became law in May of 1883.[71]

The revision of the law had a pronounced effect. The financial reports of a sample of Massachusetts communities, made up of thirty-three towns and cities, indicates that in sixteen of these communities, physicians and midwives began to report births in 1883. In eight towns, they did so in 1884, in two in 1885, in two others in 1887, in two towns in 1888, while in three communities, births were still not being returned as late as 1890. Although observance of the law was thus almost universal by 1890, a census supervised by the town or city clerk continued to be used as a means of recording births, in part to compensate for the births not attended by physicians or midwives but also to make up for the physicians and midwives who failed to report births.[72] This was true particularly in the

[68] The petition is included in the manuscript materials relating to Chapter 158 of Massachusetts, Acts and Resolves, 1883, on file in the Massachusetts State Archives, State House, Boston.

[69] Listed as the petition of A. N. Smith in the files for 1882 of the Massachusetts State Archives, State House, Boston. Also *see* Massachusetts: *House Journal*, 1882, p. 227.

[70] Petition of Henry Copes, M.D., in the files for 1883 of the Massachusetts State Archives, State House, Boston. Also *see* Massachusetts, *House Journal*, 1883, p. 204.

[71] Massachusetts, *House Documents*, 1883, Nos. 202 and 281. Also *see*, Massachusetts: *House Journal*, 1883, pp. 398 ff.

[72] *See*, for instance, the annual city reports of Taunton and Haverhill after 1885,

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larger towns and in the cities. In Taunton, for instance, in the period 1883–1886, only 53 per cent of all the births registered were obtained from the returns of physicians and midwives.[73] In Haverhill, the proportion was 70 per cent. So far as the smaller communities were concerned, the performance of physicians there was either much better or much worse than in the cities. Almost all the births which occurred between 1883–1886 in Barre, a town of 2,400 inhabitants in Worcester County, were returned by physicians and midwives. The same was true in Hinsdale, in Berkshire County, a town with 1,700 residents. On the other hand, in the towns of West Boylston and Westborough, both in Worcester County, a smaller proportion of births were returned by physicians and midwives than in Haverhill and Taunton. The variations among the smaller communities probably depended upon whether they had a resident physician or the nearest physician lived two or three towns away.[74]

Did the law of 1883 improve the completeness of birth registration? It is hard to say definitely, but I believe that it did. We know, for instance, that a similar law, passed later in the decade, brought about a marked improvement in the returns of births in the City of Boston. Boston had been specifically excluded from the provisions of the 1883 law, in spite of the fact that the editor of the Registration Reports mentioned the probability that births occurring to immigrants who used the City as a port of entry were not recorded in the semi-annual census of births.[75] On the appeal of the Boston Board of Health, this feature of the 1883 law was repealed and physicians and

which continue to list expenditures for a census of births. The Massachusetts State Library, State House, Boston, has an almost complete collection of these reports.

[73] The estimates for Taunton, like those for Haverhill, Barre, Hinsdale, West Boylston, and Westborough, were made by comparing the fees paid annually to physicians and midwives as listed in the financial reports of these communities, with the total number of births registered in these towns and cities, as listed in the annual Registration Reports.

[74] Probably one-fifth of the towns in Massachusetts had no resident members of the Massachusetts Medical Society; and about the same proportion had only one resident member. See TRIENNIAL CATALOG AND DIRECTORY OF THE MASSACHUSETTS MEDICAL SOCIETY. Boston: David Clapp, 1881.

[75] Massachusetts: Twenty-Seventh Registration Report, p. 6.

midwives in Boston were required to return births in 1889.[76] During the first few years it was in effect, the bill has virtually no influence on the returns because the Boston City Council refused to pay physicians and midwives the required fee. Only 421 births out of a total of 12,650 registered in the city in 1889 were returned by physicians and midwives.[77] The system was changed, however, beginning in 1893, through the protests of the physicians themselves,[78] and, perhaps more importantly, because of the efforts of William Whitmore, the antiquarian and genealogist and former Massachusetts Record Commissioner, who was appointed Registrar of Boston in 1892.[79] Whitmore sent out circulars to the physicians of the City advising them of the 1889 law and attempting to impress them with its significance.[80] He succeeded well enough so that of the 14,602 births registered in Boston in 1893, 10,938 were returned by physicians and midwives.

The Period 1890–1900

There were only two changes of importance in the registration system during the last decade of the nineteenth century. In 1894, the State Board of Health finally succeeded in gaining the sanction of law for the returns of deaths made directly to it by some of the towns and cities in Massachusetts. The weekly returns of mortality which the Board began to collect in 1869 from the twenty most populous communities in the State had been sent to it voluntarily. Under the new law, all towns with 5,000 inhabitants or more were obliged to file a return with the Board, on a form and according to a plan prescribed

[76] Massachusetts: *House Journal*, 1889, pp. 180 ff. Also see *Boston City Documents*, 1888, No. 17. For the text of the 1889 law, *see* Massachusetts: Forty-Seventh Registration Report, p. 417.

[77] Boston: Report of the City Auditor of the Receipts and Expenditures of the City of Boston and the County of Suffolk, 1889–1890.

[78] Boston: *City Council Minutes*, 1889, pp. 1050, 1054 and 1055. Also *see* Boston: *City Council Minutes*, 1890, p. 348.

[79] Dictionary of American Biography, xx, pp. 153–154.

[80] Boston: Annual Report of the Registry Department for the Year 1892. Boston: Rockwell and Churchill, 1893.

by it, although the returns were restricted to once a year.[81] The second change was that in 1891 all the supplementary tables and the interpretative text based on the annual returns of births and deaths sent to the Secretary of State were included in the State Health Reports instead of in the Registration Reports. The latter Reports continued to offer only the basic data, with few guides to its significance. In 1892 and 1893, and thereafter as well, the State Health Reports ignored the annual returns collected by the Secretary and dealt with the weekly mortality returns. Following the passage of the 1894 law, the State Health Reports also included a discussion of the returns made by communities with 5,000 inhabitants or more.[82] In other words, the returns made to the Secretary of State, and the Reports his department issued which were based on them, lost their singular status as the major source of published information about the trend of mortality, morbidity and the causes of death.

Neither of the changes introduced between 1890 and 1900 had significant effect in helping the registration system to achieve its basic goals, for the reason that by 1890, the main features of an adequate system had been adopted and put into operation. No more than one or two per cent of the births and deaths which occurred in the State were not registered.[83] Information relating to the age, sex, and nativity of the deceased and the sex and nativity of the parents and month of birth of the new born was recorded in almost every case. The returns of the causes of death had attained a high degree of accuracy and reliability. The data collected by the system were made available quickly and easily to consumers of vital statistics. Probably the major defect of the system was that the birth record did not include the order of the birth. Unfortunately the crucial importance for studying fertility of this item of information was not recognized at the turn of the present

[81] *See* Massachusetts: Twenty-Sixth State Health Report, p. 778.

[82] *See* Massachusetts: Fifty-First Registration Report and Massachusetts: Twenty-Fourth to Twenty-Ninth State Health Reports, inclusive, *passim.*

[83] Fisher, Irving: *op. cit.*, p. 128.

century. Now, of course, birth order is recorded on the birth certificate in every American state with the exception, oddly enough, of Massachusetts.

Although they had no influence on the development of the major features of the registration system, the law of 1894 and the changes in the form of the Registration Reports introduced in 1891 are significant because they symbolize so well the outcome of the struggle for the control of vital registration which had been going on between the Board of Health, on the one hand, and, on the other, the General Court and the department of the Secretary of State. The modern system was inaugurated in 1842 at the insistence of physicians and statisticians who recognized that the existing procedures of recording births and deaths made the records virtually useless as data for studying the condition of the public health. The department of the Secretary of State was put in charge of registration. It did a fairly good job of getting the system established: by the 1850's, all the communities in the State were making returns of births and deaths and no more than fifteen per cent of the deaths and less than twenty per cent of the births were not registered. But the causes of death were often given inaccurately in the returns, and besides, the Registration Reports, which summarized them, did not make the best possible presentation of the available data. The physicians and the statisticians had a simple explanation of these faults: the system was being run by laymen unfamiliar with medical matters. The proper therapy which seemed to follow from this diagnosis was equally obvious: put medical personnel in charge of the system. The sanitary movement was gaining public support during the 1850's and 1860's. The movement found its expression in the agitation in behalf of the formation of a State Board of Health. The thought occurred to the critics of registration that the most perfect solution for the weaknesses of the system would be to have the Board of Health take charge of registration. The General Court and the Secretary of State, however, did not view the matter in the same way, and although they finally set

up the Board of Health in 1869, they kept the registration system under the authority of the Secretary of State. From the point of view of the General Court, it seemed as if vital records were still primarily legal documents, and, therefore, they ought to be collected and handled by the same department as collected other official documents of the towns and cities. After 1869, the physicians and statisticians, working through the Board of Health, conducted an incessant campaign to minimize the consequences of the decision of the General Court. Making use of its advisory and consultative powers bearing upon the public health, the Board gradually was able to assume many of the responsibilities for the operation of the system which traditionally belonged to the department of the Secretary. The Board made investigations of the operation of registration, it initiated legislation designed to reform the system, it obtained authority for the local boards of health to supervise death registration in the towns and cities, and the Secretary of the Board even became the editor of the Registration Reports. I have called the events of the 1890's symbolic, because having had no effect on the attainment of the basic goals of registration, they are meaningful only as a sign of the successful campaign conducted by the physicians and the statisticians. The outcome of the campaign was satisfactory to both physicians and the government, for the reason that it preserved the conception of the birth and death records as important legal documents and at the same time it enabled the records to be used to the maximum degree as statistics for studying the public health. What better finale could there have been to the history of birth and death registration in Massachusetts during the nineteenth century?

Appendix

At several places in this series of papers, I have offered generalizations about the response of towns and cities to the registration law that were based on a study of the financial or town reports of a representative group of communities. To the reader trained in demography and the behavioral sciences, a phrase like "representa-

tive group" is irritating in its ambiguity and, therefore, I think it is appropriate to comment on the nature of this group of communities and why I chose to study it.

To answer the latter question first, I would like to point out that as early as 1840 there were over 300 towns and cities in Massachusetts and that by the end of the century, there were exactly 352 separate communities in the State. In dealing with the impact of the registration laws on the work of clerks, undertakers, physicians, and boards of health, it would have been impossible to study their effect in each one of the 300 towns and cities. Not only because of the burden in terms of time and labor complete coverage would have imposed, but also because financial reports and other sources were available for only a portion of the communities, I decided instead to examine only a group of towns and cities in detail. In so doing, I hoped not merely to simplify my task. I also wanted, *insofar as was possible,* to avoid the kind of crude generalization represented by statements like "most of the towns did thus-and-so" or "in general, the response was positive"—the sorts of statements that one usually finds in the writings of conventional historians.

The nature of the sample chosen for study is perhaps best made evident by describing the manner in which it was formed. The universe from which the sample was drawn consisted of 304 towns and cities listed in the First Registration Report. Altogether 311 towns and cities were listed in this Report, but I decided to exclude seven of them, as follows: Boston, Lowell, and Salem (omitted for the reason that they were the dominant population centers in the State and I wanted to include them in the sample, whether or not they turned up by means of the procedure used); Charlestown and Somerville (not included because their vital statistics were tabulated together in the early Registration Reports); and Blackstone and Ashland, two small towns (they were excluded because they had not been established at the time of the 1840 census, and therefore no population figures were available for them until 1850).

The 304 towns and cities in the universe were stratified into six sub-universes. The six sub-universes were: towns of 000–799 inhabitants; towns of 800–999 inhabitants; towns of 1,000–1,499 inhabitants; towns of 1,500–2,199 inhabitants; towns of 2,200–3,299 inhabitants; and towns and cities of 3,300 inhabitants or more. A table of random numbers was used to construct the sample list of towns

from each sub-universe. Although my goal was a sample of thirty-five to forty towns equally distributed among each of the six sub-universes, a list was drawn of one hundred towns distributed fairly equally among the sub-universes. A large list was used initially because I wanted the towns in the final sample to be communities which could meet certain criteria. For instance, I preferred towns and cities for which published reports of town expenditures going back into the 1840's and 1850's would be available. I preferred communities with good town histories written about them. I also wanted to be sure that a certain proportion of the towns in the final sample would be included among the communities whose vital records had been printed in the series of genealogical records dealing with the years before 1850, published by the New England Historic Genealogical Society and other historical societies in Massachusetts. A preliminary study of these sources had revealed that, at the most, only one-half the towns and cities in the State had such materials available about them. It was necessary, therefore, to begin with a list of one hundred communities in order to compensate for the loss of sample candidates that would occur inevitably when these criteria were applied to communities chosen at random from the sub-universes.

My final decision was to try to develop a sample of thirty-six towns and cities, plus the communities of Boston, Salem, and Lowell. This sample was to include six towns with 000–799 inhabitants; six towns with 800–999 inhabitants, and so on, until all the sub-universes were represented in the sample. The actual selection of towns and cities proceeded in the following way. In order to reduce the list of one hundred towns, I began by choosing six towns at random from each of the sub-universes. I then checked each of these six towns against the available sources in the Massachusetts State Library, State House, Boston; and in the Widener Library, Harvard University, Cambridge, to see whether it met the criteria I had established. If the community did meet the criteria, it was admitted to the sample. If the town or city failed the test, it was discarded and another community was selected *at random* from the appropriate sub-universe in the list of one hundred towns, until towns and cities from each sub-universe were found which met the standards of the sample.

The available sources proved to be even less adequate than the

preliminary canvass of the materials had indicated, and consequently there were towns admitted to the sample although they did not meet all the criteria which were adopted at first. Indeed, so poor was the quality of the sources that I could find only five towns in the list of one hundred that could be included in the sample as representatives of the sub-universe of towns having 2,200–3,299 inhabitants. Seven communities were chosen for the sub-samples of towns with 1,000–1,499 inhabitants and 1,500–2,199 inhabitants. These sub-samples included one more than the usual six towns for the reason that unusually good source material was available for a seventh community. As a consequence of these various procedures and decisions, the final sample included thirty-seven towns, plus Boston, Salem and Lowell; or a total of forty towns and cities. The full list of the communities in the sample is given below.

A. Towns with under 800 *Inhabitants in* 1840

1.	Leyden	Franklin	County
2.	Montgomery	Hampden	"
3.	Easthampton	Hampshire	"
4.	Boxborough	Middlesex	"
5.	Burlington	"	"
6.	Lincoln	"	"

B. Towns with 800–999 *Inhabitants in* 1840

7.	Hancock	Berkshire	County
8.	Hinsdale	"	"
9.	Bedford	Middlesex	"
10.	Tewksbury	"	"
11.	Wayland	"	"
12.	Medfield	Norfolk	"

C. Towns with 1,000–1,499 *Inhabitants in* 1840

13.	Brighton	Middlesex	County	(statistics merged with Boston 1874)
14.	Bellingham	Norfolk	"	
15.	Brookline	"	"	
16.	Foxboro	"	"	
17.	Walpole	"	"	
18.	Northbridge	Worcester	"	
19.	West Boylston	"	"	

D. Towns with 1,500–2,199 *Inhabitants in* 1840

20.	Norton	Bristol	County
21.	Reading	Middlesex	"
22.	Milton	Norfolk	"
23.	Bridgewater	Plymouth	"
24.	Ashburnham	Worcester	"
25.	Lancaster	"	"
26.	Westborough	"	"

E. Towns with 2,200–3,299 *Inhabitants in* 1840

27.	Lee	Berkshire	County
28.	Westport	Bristol	"
29.	Amesbury	Essex	"
30.	Abington	Plymouth	"
31.	Barre	Worcester	"

F. Towns with 3,300 *or More Inhabitants in* 1840

32.	Adams	Berkshire	County	
33.	Taunton	Bristol	"	
34.	Beverley	Essex	"	
35.	Haverhill	"	"	
36.	Springfield	Hampden	"	
37.	Roxbury	Norfolk	"	(incorporated in Boston 1867)
F. 38.	Lowell	Middlesex	County—20,796	inhabitants in 1840
39.	Salem	Essex	" 15,082	"
40.	Boston	Suffolk	" 93,383	"

Some communities are included in the sample for which town reports were not published until after the inauguration of the modern registration system in 1842. This fact will explain why generalizations in the text of the four papers are often based on data drawn from fewer than forty towns and cities. In other words, the appropriate information was not available for certain towns early enough to answer various questions posed in the series of papers. In some cases, although town reports were printed, they were not sufficiently detailed to provide answers to the questions which concerned me.

ANNOTATIONS

From Malthus to Mao Tse-Tung[1]

Readers of this book may be disappointed if they expect to find a searching discussion of population theory and policy in Communist China, supported by comprehensive documentation. Their disappointment will, however, be short-lived because Professor Sauvy has written what this reviewer considers one of the most stimulating expositions of the "population problem" in any language. It is to be hoped that translations into English and other languages will soon make the book accessible to the non-French-reading public.

De Malthus à Mao Tsé-Toung is arranged in three major parts, followed by a brief overview and conclusions in the final chapter. The first part of the book, entitled "Facts and False Solutions," focusses attention on "*the new fact that after a long period of quasi-stagnation, the numbers of people in the greater part of the world have entered a phase of rapid growth*" (p. 22, Sauvy's italics in all quotations), reflecting a combination of declining mortality with constant fertility in many countries of Asia, Africa, and Latin America.

Sauvy asks two crucial questions. Can rapid growth continue for a long time? Should one be concerned about it? His answer is "no" to the first question, and "yes," to the second. It is unlikely that the decline in mortality can be reversed since governments will find it "*impossible to stop medical progress from spreading, at least in its summary and collective techniques*" (p. 91). Nor will the economically developed countries be able to withhold aid if millions of human beings are threatened by

[1] Sauvy, Alfred: De Malthus à Mao Tsé-Toung. Paris, Editions Denoël, 1958, 303 pp., 900 fr. + T.L.

local famines. Emigration can alleviate overpopulation only in small territories, mainly islands politically associated with larger countries. "Thus we find ourselves in a grand dilemma: *economic solution or demographic solution.* Adjustment of resources to population or adjustment of population (through declining natality) to resources. *There is no escape hatch*" (p. 104).

The "economic solution" is discussed in the second part of Sauvy's book. He points out that a population growing at an annual rate of 2.5 per cent must make "demographic investments" of about 10 per cent of its national income merely to maintain its level of living. The relatively high proportion of children in a rapidly growing population also tends to reduce goods and services available for consumption per adult equivalent. To improve the level of living, additional "economic investments" must be made, necessitating further belt-tightening. Sauvy concludes that "*many underdeveloped countries are experiencing too rapid population growth to succeed in getting out of the hole by their own efforts*" (p. 153). The participation of foreign funds, private or public, in economic development is often blocked by political considerations. It is not surprising, then, that in recent years economic progress has been very slow or even lacking, especially in densely settled countries, such as India and Egypt, and that hunger and misery have persisted.

Sauvy then turns to the "demographic solution." He traces the history of birth limitation—incidentally, a much-neglected subject. He analyzes the social and psychological factors both promoting and impeding the widespread adoption of contraceptive practices and points to the political implications in the atomic age. "The Malthusian reaction of familial and national paternalism recurs in colonial paternalism; from the moment when subjects become charges, the growth of their numbers becomes a matter of concern; it appears desirable to limit, if not the number itself, at least the rate of growth.

"These preoccupations . . . manifest themselves on an even larger plane: the fear of world overpopulation. As soon as the commercial, if not political, domination by the West ceased to be absolute . . . the fear of excessive numbers was bound to grip

the mind" (p. 225). Sauvy has some harsh words to say about the manner in which this fear has occasionally been expressed, especially by Americans.

In a concluding chapter, Sauvy stresses once more the urgency of the situation and the necessity of employing all economic and demographic remedies. While up to now neither the U.S.A. nor the U.S.S.R. has faced up to the vast task ahead, the problem offers a common challenge to both protagonists of the cold war and to all humanity.

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THE SCIENCE OF DEMOGRAPHY

THE National Science Foundation in its Second Annual Report (1952) expressed the need for "an adequate body of fact about the current status of science in the United States, including an inventory of our present resources in trained men and facilities." Such a need could be met only through studies of the individual sciences and one such study which the Foundation supported was that of demography.[1] Major responsibility for the study was concentrated at the University of Chicago Population Research and Training Center. Philip M. Hauser and Otis Dudley Duncan of the Center staff edited and also contributed substantial sections to the publication. Twenty-seven other authors wrote individual chapters.

The book opens with a brief preface and a comprehensive "Overview and Conclusions" by Hauser and Duncan which serve to orient the reader to the scope, nature, and methods of the undertaking. These are followed by the four major sections of the book: Part I, Demography as a Science, by Hauser and Duncan; Part II, Development and Current Status of Demography; Part III, Elements of Demography; Part IV, Population Studies in Various Disciplines. The last three parts are composed of individual chapters by various authors, with

[1] Hauser, Philip M. and Duncan, Otis Dudley (Editors): THE STUDY OF POPULATION, AN INVENTORY AND APPRAISAL. The University of Chicago Press, Chicago, 1959, 864 pp. $15.00.

an introduction by the editors to each Part. It is impossible to discuss these chapters individually, but in order that this review may be informative as to the coverage and contributors, the Table of Contents is appended.

A serious problem faced by every editor of symposia is that of achieving some unity to the undertaking. The editors of the present volume unified the work through the extent of their own writing, through the "Overview and Conclusions" which pulls the whole volume together, and through the instructions to the individual contributors to guide them in the organization of the material desired. Although there is considerable variation in the extent to which the suggested outline was followed, this guide must have been an enormous aid as the contributors approached assignments any one of which could easily have developed into a book of its own. Included in the guide was a request for a selected bibliography. Since the chapters range widely over the whole field of demography, the selected bibliography prepared by each author for his subject may well prove to be one of the most useful features of the book.

In the "Overview and Conclusions" the editors consider at some length the question as to what constitutes the science of demography, debating the merits of the restricted definition of demography as synonymous with demographic analysis, versus the more comprehensive definition embracing all population studies. They argue persuasively for the more limited definition as designating a single theoretical discipline with a coherent frame of reference. Under the term "population studies" they include the correlation of demography so-defined with the subject matter of related sciences. Out of this approach they give a working definition that could certainly serve as a basis for the entire discussion of demography as a science. It is disappointing, however, to find that they do not follow through consistently on their excellent start in bringing order to the subject matter that is usually embraced under demography. Indeed within the "Overview" itself, they use the word demography in both the strict sense as they have defined it and in the all-embracive sense so commonly met. It is hoped that this attempt to formalize the definition of demography will be pursued for its continued use in the unlimited sense is handicap-

ping to the development of the subject of demography as a science.

Taken as a whole, the editors have covered the field comprehensively and have done a skillful job in maintaining unity in a volume that is essentially encyclopedic in character.

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LOWELL J. REED

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THE FAMILY AND POPULATION CONTROL[1]

PUERTO RICO has been a demographic puzzle for some time. At least since Paul Hatt's study in 1948 there has been evi-

[1] Hill, Reuben J.; Stycos, J. Mayone; and Back, Kurt W.: THE FAMILY AND POPULATION CONTROL: A PUERTO RICAN EXPERIMENT IN SOCIAL CHANGE. Chapel Hill: University of North Carolina Press, 1959. Pp. xxvi and 481. Price: $8.00.

dence that most Puerto Ricans espouse small family ideals. Yet Puerto Ricans obviously have not acted to implement these ideals: family size continues to be large, government birth control clinics are used by relatively few, and, as the study under review indicates, most people who use birth control are likely to begin late and to use it ineffectively. Sterilization is used by a relatively large minority of couples but usually only after the desired family size has been surpassed. The net result is a continuation of high birth rates despite considerable success in a "bootstrap" program for economic and social development.

Hill, Stycos, and Back have stated the resulting research problem as follows: "With the overwhelming evidence that Puerto Ricans are small-family minded, the research problem becomes one of accounting for the over-achievements in reproduction—the discrepancy between stated family size goals and achievements."

In their attempt to solve this complex problem, the authors have made a notable contribution to our knowledge of the dynamics of fertility in an area in transition from an underdeveloped status. Their study will be a standard reference in this field for many years to come.

The authors have used a complex research design in an attempt to winnow from a rather long list of possible causal variables the smaller number which could give a comprehensible model of the basic forces at work in this situation. While many important specific findings are developed, we are left with considerably less than a definitive explanation. The total amount of variance in fertility planning explained by the variables in the model developed is very modest. This does not result mainly from any lack of ingenuity or skill on the part of the investigators. In the reviewer's opinion, this is inevitable in the current status of the social sciences when data to test a complex theoretical model are collected with reference to a large variety of sociological and psychological variables, each of which must be measured with primitive (and usually *ad hoc*) scales, and then interrelated with statistical devices whose mathematical assumptions are met rarely by the variables as measured. Given the basic tools available the authors are to be

congratulated on the clarification they have been able to give to a complex theoretical problem.

The overall study is divided into three stages: (1) an intensive preliminary reconaissance based on 74 cases (reported by J. M. Stycos in his FAMILY AND FERTILITY IN PUERTO RICO[2]) (2) an extensive sample survey analysis to test the relationship of a large number of variables to each other and to fertility and family planning, and (3) an experiment in which key variables selected from earlier studies are manipulated to try to induce changes in fertility planning and fertility.

Two samples are used in the stage of survey analysis. For a few of the questions, a probability cross section of the total Puerto Rican population was available. However, most of the analysis rests on a purposive stratified sample (OPD sample) drawn from visitors to Out Patient Departments of government medical clinics. This stratified purposive sample as well as the sample for the experimental stage is drawn so as to include only rural, low status huband-wife couples married long enough to have proven their fertility and to have faced family growth problems.

The OPD and experimental samples have several limitations which deserve comment. The OPD sample is admittedly neither a probability sample nor a weighted sample of the population which it is supposed to represent. The authors justify this by saying; "at this point we were primarily interested not in the actual quantities (numbers and percentages) but in the interrelationships with antecedent variables. No weighting of the final results to account for the different sampling ratios from strata was attempted." Apparently the authors subscribe to the position that relationships can be studied without the probability samples required for estimating proportions or numbers characteristic of a particular historical population. However, even "basic relationships" also are subject to sampling variability in whatever universe they are presumed to exist. Unless the authors can assume that all the other variables not held constant do not affect the relationships the results will tend to vary with the sample selected, so a probability

[2] Stycos, J. Mayone: FAMILY AND FERTILITY IN PUERTO RICO: A STUDY OF THE LOWER INCOME GROUP. New York, Columbia University Press, 1955, 332 pp. $6.00.

sample design is just as pertinent for a correlation as for a proportion in a specific population. (The authors recognize this implicitly in applying significance tests to correlations and other measures calculated from the OPD sample.) Without reference to the question of representing the whole Puerto Rican population, if the relationships are presumed to be valid for the particular subgroup of the population studied, the samples should be drawn on a probability basis from this subgroup, however defined. The reviewer recognizes the difficulties in following this rule in a pioneering field study. The authors are probably quite justified in using a purposive sample for exploratory purposes, but its limitations must be explicitly recognized, especially when the words "definitive" and "conclusive" are used with reference to some of the results.

The authors compared the characteristics of the OPD sample and census data and found that the OPD sample was younger, somewhat better educated and more fertile than the census comparison group. However, they conclude that "the differences on none of the parameters are striking nor likely to make unacceptable judicious application of study findings to Puerto Rico as a whole." This may be true, but with a purposive sample it is not clear just how one interprets judiciously the particular deviations that occur. Especially troublesome is the fact that there is no assurance with such a sample that the deviations may not be different in kind or size for the variables that cannot be checked against outside data. Some further uneasiness about the possible selectivity of the sample arises from the fact that the OPD sample has a considerably higher rate of use of contraception and uses more effective methods than a cross section probability sample.

Another technical sampling problem is the use of sampling tests which are based on the concept of a simple unrestricted random sample for clustered, purposive samples. Assuming that the probability assumptions are not seriously violated, the fact that samples involve clustering means that the tests used will tend to exaggerate the level of significance of some of the relationships reported.

A final reservation refers to the universe defined for study rather than the technical problems of sample selection within

the universe. The authors have chosen to focus on low status rural groups for sound reasons: this is the crucial problem group and is a very large part of the Puerto Rican population. However, this design minimizes the possibility of studying broad differences in socio-economic structure as between lower class and middle class Puerto Ricans. The authors conclude at several points that broad structural variables are probably of less explanatory importance than intra-familial organizational variables. But this may be, at least in part, a result of the focus of the sample on low-status families. (The authors, themselves, later use the homogeneity of the sample as one basis for explaining the relatively low multiple correlations achieved).

Turning now to the substantive findings, a very valuable result is the negation of a set of stereotypes about why Puerto Ricans have not effectively achieved their small family ideals: that Puerto Rican males insist on high fertility to demonstrate their virility as part of a cult of "machismo," that the lower classes are ignorant of birth control methods, that the influence of the Roman Catholic Church is a deterrent to effective fertility planning, and that the lower classes really have large family values. The evidence contradicts all of these stereotypes.

Another significant survey finding is that the professed small family ideals are often vague and held with considerable ambivalence. The authors are very ingenious in using measures of inconsistency and ambivalence about family size as a variable. They show with considerable plausibility that the ambivalence is not simply an artifact of the survey method but exists in the groups studied and is genuinely related to ineffective fertility planning.

It is impossible in a review to summarize all of the important and provocative survey findings. The following is a very rough general picture which emerges after many variables are considered: Puerto Ricans have small family values, but these are held vaguely and with ambivalence especially early in married life. Early in marriage knowledge about birth control is either vague or not shared by husband and wife in a way to permit effective joint planning. Modesty on the part of the wife and her respect for her husband together with poor communication between them and other manifestations of poor family organi-

zation result in such a low degree of empathy that even when husband and wife have similar values they may not know that this is true. The couples do not have the kinds of interpersonal relations required for jointly developing and carrying out long range family plans. As a result, the desired family size is likely to be surpassed before the family takes effective action as a unit. In this extremity the emergency action is likely to be sterilization. The late timing of serious consideration of family planning is very important in explaining high Puerto Rican fertility.

The authors believe that the institutions formerly supporting high fertility are no longer operative so that "family planning is neither hindered nor supported by institutional patterns and adherence to cultural norms." Therefore, they conclude that much depends on the individual family and its competence in intrafamilial communication and joint action. In the authors' words: "competence in problem solving in Puerto Rico's changing society rests on a flexible family organization infrequently found in our sample, characterized by full communication between spouses on all key marital issues, close rather than distant relations, freedom of the wife to work gainfully, and to participate socially, and high agreement on the major issues of marriage and parenthood."

The study indeed does provide considerable evidence that intrafamilial organization is related to effective fertility planning. Whether it deserves to have the central position which the authors give it in their explanation of the current situation and in their program for action is difficult to determine from the evidence presented. In the first place, the correlation of the family organization variables with family planning, although higher than for most variables considered, still leaves the greater part of the variance of fertility and family planning unexplained. Secondly, as already noted, the sample design is such as to minimize the possibility of studying broad structural and socio-economic variables. In the third place the authors chose to work with a model emphasizing intrafamilial interaction and minimizing the structure-function approach. Relatively little attention is given to the role of the family in the larger community and society and the possible continuation of

important extended family functions in the lower status group. It is not impossible that the Puerto Ricans have adopted a stereotyped small family ideal before economic and social development of the country has provided institutional substitutes for the large family. The authors are perfectly justified in concentrating on the intrafamilial interaction variables if they wish, and their approach did yield rich results. It is much less certain that they are justified in drawing conclusions about the central importance of the intrafamilial interaction processes when they have such little empirical material on the position of the family in the community and the society.

The reviewer cannot help but wonder whether any of the Western low fertility countries have now or had during their demographic transition (e.g. late Victorian England) the "full communication between spouses on all marital issues" or the "high agreement on the major issues of marriage and parenthood" which the authors see as necessary for effective fertility planning under Puerto Rico's conditions of social change. Perhaps, such perfection is more frequent in the handbooks of marriage counsellors than in the mass of the population of low fertility countries.

The experimental stage of this study is bold, unique, and, in many ways, brilliantly contrived. Briefly, the authors chose a group of low status rural villages and in preliminary interviews selected stratified matched subgroups on the basis of information about family planning, values about family planning, and type of family organization skills. All potential respondents were not regular users of effective birth control methods at the time. One group of villages was set aside as a control. The experimental villages were divided for a variety of treatments. In one group the stimulus was applied entirely by the distribution of pamphlets. In the others the stimulus was a group of three small-groups discussion meetings, involving visual aids, role-playing, and other educational devices. Villages receiving each method of presentation were further subdivided so that the content emphasized values in some, family organization skills in others, and in still others, a combination of the two. In all experimental villages information was given about family planning methods. All families, including the con-

trols, were interviewed before the experiment, one month afterwards, and then a year later to check on the actual fertility. The experimental goal was to induce non-users to begin birth control and irregular users to improve their practice.

In general the pamphlets were most effective in initiating use and small group discussions in improving the level of use. The percentage increase in use and effectiveness in the experiment is remarkably high, reaching 50 per cent in some subgroups.

Even more remarkable is the fact that 24 per cent of the non-users in the control groups reported beginning birth control during the short experimental period. The authors interpret this to mean that many Puerto Rican couples are so "ready" for family planning that even a small stimulus, such as the pre-interview, is enough to "trigger" the action. Another interpretation of at least part of this increase is also plausible, although less favorable to the experiment: some of the couples (both control and experimental) may have learned in the pre-interview what kinds of responses would please the interviewer and then obliged with appropriate responses, in the post-interview. If the couples are so ready that a short non-directive interview will lead them to initiate contraception it is difficult to understand why there has not been more general use of contraception in Puerto Rico in view of the widespread discussion of the problem on the Island and the existence of a network of birth control clinics.

One of the most important conclusions the authors draw from the experimental (and other) data is that relatively small stimuli can lead to the initiation of birth control but that to sustain practice at a high level over the long child-bearing period, persistent supporting pressures and the existence of effective family organization are required.

Difficulties arise in evaluating some of the specific results of the experiment because large numbers of potential respondents in the small group discussion experiments did not attend one or more of the meetings. Only 59 per cent of the men and 40 per cent of the women attended even one of the group sessions and only 8 per cent of the men and 6 per cent of the women attended all three.

In a final chapter the authors present a set of recommenda-

tions for a "crash program" for Puerto Rico to narrow the discrepancy between the actual fertility and the small family ideals. The main features of the program include (1) a massive educational program through health and other agencies to keep the reasons for and the facts about family limitation persistently before the population, particularly in the first year of marriage and after each birth. This recommendation follows from the research finding that persistent stimuli are needed to maintain regular family limitation practices. (2) A program for a very extensive program of training in the skills of family communication and organization through schools and other agencies which reach individuals before and families after marriage. (3) Removal of some of the barriers to utilization of the birth control clinics, discovered in the research. The authors discount as a significant part of such a program major changes in the broader social and economic organization as "neither feasible nor particularly desirable in view of the large scale societal reorganization involved." The reviewer is not convinced that this judgment is correct. He is also dubious that a mass program of education in family living is as easy to carry out as the authors imply.

In a study of the scope of this one it is inevitable that there will be many points at which critical statements are possible and necessary. I have expressed in this review some serious reservations about some parts of the study. The authors have made such criticism possible by a frank and comprehensive presentation of their methods and problems. Field workers will learn much from a careful study of these materials. The great wealth of significant substantive findings will be an important basis for replication and further research.

RONALD FREEDMAN

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